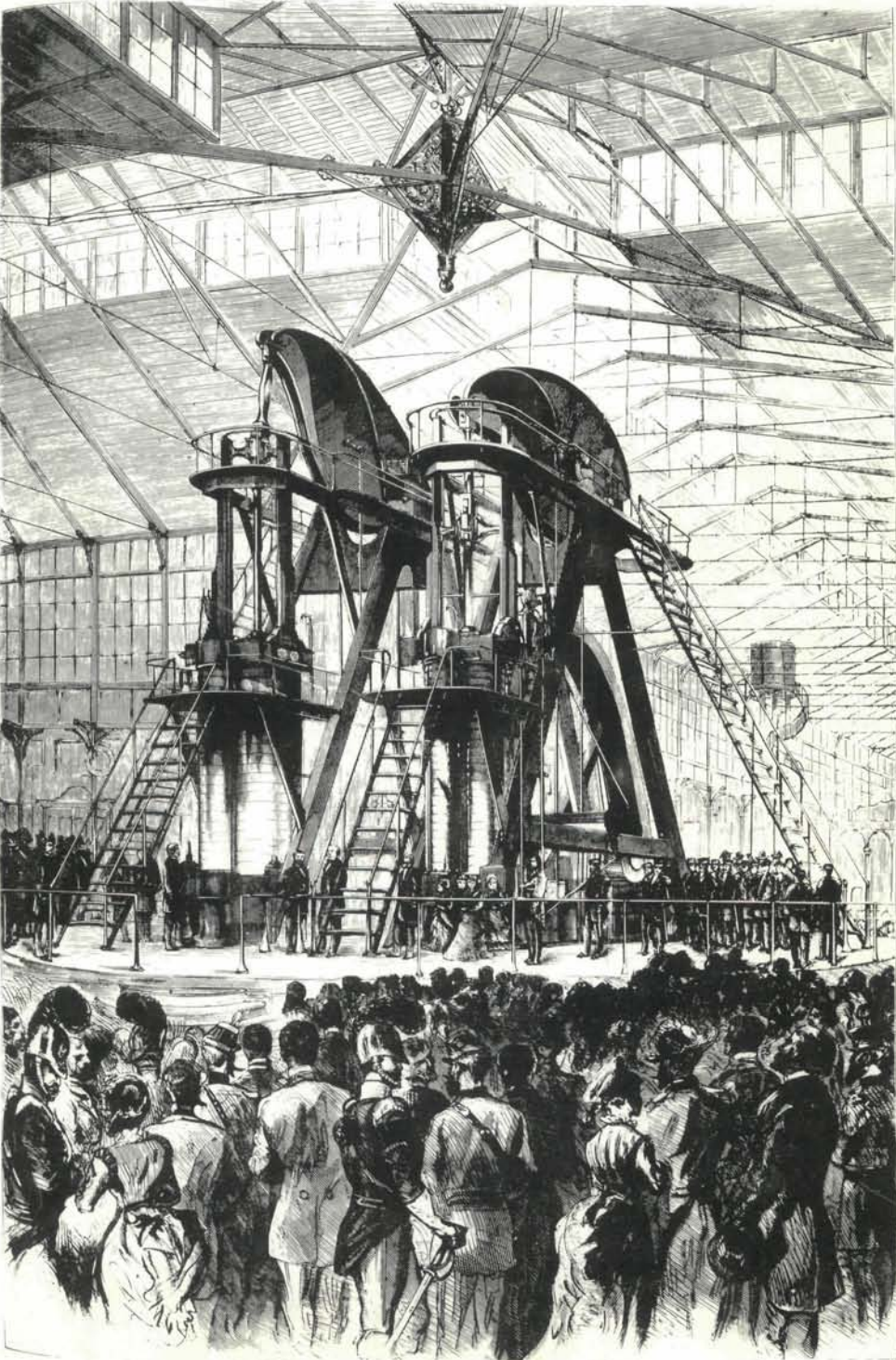


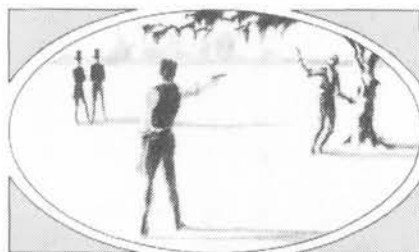
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Reassessing the Power Patterns of the Industrial Revolution: An Anglo-American Comparison

DOLORES GREENBERG

FROM RESEARCH NOW BECOMING AVAILABLE, it appears that historians have long been mistaken in their assumptions about the power patterns of England's Industrial Revolution. And, captured by our notions of the Age of Steam, we have constructed misleading paradigms for analyzing the dynamics of development. Rather like Thomas Boulton, James Watt's partner, who boasted, "I sell here . . . what all the world desires to have—POWER," we have presumed too much for Watt's wondrous invention while neglecting the expanding contribution of traditional power sources to increasing productivity.¹ Contrary to commonly held suppositions about the rapid spread and use of steam, both Great Britain and the United States depended to a striking degree on renewable, "soft energy" sources. In the two countries credited with the earliest and greatest industrial progress, the transition to fossil-fueled power supplies for machine technology was uneven and prolonged. This discovery compels us to reassess the significance of steam before 1850 as part of a broader reappraisal of the nature and extent of energy innovation during the Industrial Revolution. More than that, the characteristics that mark this

I want to thank my colleagues in the Energy Policy Studies Seminar at Hunter College, City University of New York, for provocative discussion of energy systems, and especially Laura Randall for her comments on an earlier version of this essay. The suggestions of an anonymous referee for the *American Historical Review* were particularly useful.

¹ Boulton, remark to James Boswell, in Boswell's *Life of Johnson*, as quoted in John F. Kasson, *Civilizing the Machine: Technology and Republican Values in America, 1776–1900* (New York, 1976), 22. That the "widespread use of steam engines" after 1782 had "changed the face of British industry" has been the classic model for prescribing policy to today's less developed countries. See Peter Lane, *The Industrial Revolution: The Birth of the Modern Age* (London, 1978), 6, 236. A. E. Musson initiated critical analysis of the "generally recognized" view that the "introduction of steam power was a crucial factor in the Industrial Revolution"; Musson, "Industrial Motive Power in the United Kingdom, 1800–1870," *Economic History Review* [hereafter, *EHR*], 2d ser., 29 (1976): 415–39. This standard interpretation has also been accepted by American historians, who have, nevertheless, noted two points of contrast in U.S. development before 1850: first, that steam's significance in transportation outweighed its use in American manufacturing, and, second, that waterpower was widely used in industry. See, for example, Nathan Rosenberg, *Technology and American Economic Growth* (New York, 1972), 59–86; Louis Hunter, *A History of Industrial Power in the United States, 1780–1930*, volume 1: *Waterpower in the Century of the Steam Engine* (Charlottesville, Va., 1980), 160, 539; and Peter Temin, "Steam and Waterpower in the Early Nineteenth Century," *Journal of Economic History* [hereafter, *JEH*], 26 (1966): 187.

era may force us to re-evaluate the ways in which energy development functions as a force for social and structural change.²

Such a reassessment is certainly due considering that increased power output has been regarded, at least since the nineteenth century, as one of the prerequisites for industrialization. Speculation about the importance of power and the nature of work as institutional phenomena in fact had paralleled, and was often provoked by, the intensifying search by scientists, mechanics, and even manufacturers to understand what energy was and what occurred when energy was converted from one form to another. Formulation of the first and second laws of thermodynamics by the 1860s not only set forth the principles that govern all energy transformations in the universe but also established that work and power are intrinsic to physical systems—indeed, to whatever happens in the material world.³ By extension, scientific theory seemed to support the premise that power is a causal agent integral to social transformation. So in Marx's *Das Kapital* power production is a key to economic and hence social change, while in Arnold Toynbee's pioneering study of the Industrial Revolution the introduction of steam is identified as the catalyst for developments of profound social consequence.⁴

In the century since Toynbee first focused on the significance of steam, the assumptions about the power scaffolding of the Industrial Revolution have remained largely unchallenged, despite the lack, until recently, of solid historical

² Soft energy, a term much used by ecologists, planners, and economists, refers to the choice of renewable, environmentally benign energy sources—sunlight, wind, water, vegetation, and organic waste. Denis Hayes, among others, has advocated a "soft energy path" to a new energy era to reduce current dependence on nonrenewable fossil fuels; see Hayes, *Rays of Hope: The Transition to a Post-Petroleum World* (New York, 1977). For two thoughtful, important studies of energy alternatives for the future, see Ford Foundation, Energy Policy Project, *A Time to Choose: America's Energy Future* (Cambridge, Mass., 1974); and Amory Lovins, *Soft Energy Paths: Toward a Durable Peace* (Cambridge, Mass., 1977). In addition to these works, which relate energy flows to modern institutional change, a body of literature by anthropologists, sociologists, and ecologists links the "evolution" of social structures to control over energy resources and power technology. See, for example, William R. Burch, "Resources and Social Structure: Some Conditions of Stability and Change," *Annals*, 389 (1973): 27–34; Lewis Mumford, *Technics and Civilization* (New York, 1934); William Frederick Cottrell, *Energy and Society: The Relation between Energy, Social Change, and Economic Development* (New York, 1955); Leslie A. White, *The Evolution of Culture: The Development of Civilization to the Fall of Rome* (New York, 1959); Marshall D. Salins and E. R. Service, *Evolution and Culture* (Ann Arbor, 1960); Dean Sheils, "The Importance of Agriculture from the Perspective of Neoevolutionary Theory," *Rural Sociology*, 37 (1972): 167–88; and R. J. Forbes, *The Conquest of Nature: Technology and Its Consequences* (New York, 1968). Historians have yet to examine these conclusions about historical change, progress, and energy, but, for some of the questions raised and an emphasis on U.S. reliance on traditional energy sources, see Dolores Greenberg, "Energy Flow in a Changing Economy, 1815–1880," in Joseph R. Frese and Jacob Judd, eds., *An Emerging Independent American Economy, 1815–1875* (Tarrytown, N.Y., 1980), 29–58.

³ Based on a fusion of Democritean atomism and Newtonian mechanics, this seminal scientific synthesis of the modern era set forth the laws governing all energy flows, defined energy as the ability to do work, and specified that power is the rate at which work is done. Instead of theory preceding technology, scientific discovery had followed from efforts to solve very practical questions about the work processes of people, animals, and machines. Sadi Carnot's investigations into how to improve the efficiency of the steam engine, for example, produced a critical paper that established the basis for the Second Law of Thermodynamics; Carnot, *Reflections on the Motive Power of Heat and on the Machines Fitted to Develop This Power* (1824), trans. R. H. Thurston (New York, 1943). For excellent introductions to thermodynamics, which emphasize its historical development, see Stanley W. Angrist and Loren G. Helper, *Order and Chaos: The Laws of Energy and Entropy* (New York, 1967); and D. S. L. Cardwell, *From Watt to Clausius: The Rise of Thermodynamics in the Early Industrial Age* (Ithaca, N.Y., 1971). Also see A. E. Musson, ed., *Science, Technology, and Economic Growth in the Eighteenth Century* (London, 1972); and Musson and Eric Robinson, *Science and Technology in the Industrial Revolution* (Manchester, 1969).

⁴ Marx, *Capital: A Critique of Political Economy*, ed. Frederick Engels (4th edn., New York, 1906), 368–556; and Toynbee, *Lectures on the Industrial Revolution in England* (London, 1884).

evidence on such fundamentals as the timing and degree of the shift to mechanization, the expanding use of conventional sources of power, and the demand for new alternatives. In classic studies by Paul Mantoux and T. S. Ashton, and in more recent major works by Walt W. Rostow and David S. Landes, England's adoption of steam technology functions as the critical component in the change from rural, slow-growth agrarianism to urban, sustained-growth industrialism. Rostow's redating, which realigned the starting point of rapid economic development with the commercialization of Boulton and Watt's engine, especially strengthened the causal link to steam. And, just as the 1780s became generally accepted as the point of take-off for steam power, so rapidly increasing dependence on steam during the first half of the nineteenth century was viewed as responsible for the basic alterations in manufacturing techniques, economic organization, and industrial location that we commonly associate with the Industrial Revolution. After a considered evaluation of the persistence of traditional power systems, Landes observed that coal and steam had "permitted" the "extraordinary development and diffusion" of the Industrial Revolution.⁵ Indeed, what was thought to be the English experience of steam-generated growth has been treated as a model that, supposedly, was later emulated, with some variation, in the United States, in Germany, and then in Western Europe.

In most scholarly writing, steam is virtually synonymous with productivity, serving in some constructs as the single most important stimulant to economic growth. Presented as an agent of economic maturation, as proof that nature could be controlled in the service of human welfare, steam also appears as a symbol of progress that, in a larger sense, provides a unifying metaphor for modernization. Not surprisingly, in an era of growing technological determinism, the advent of steam has supplied the theoretical crux for neoevolutionary analyses of inclusive and definitive transformation—the change from handicraft, "man-made," muscle-powered methods of production to more advanced, modern, mineral-fueled, power-driven mechanization.⁶ Even historians who have acknowledged the continuity of traditional techniques have presumed such processes to be part of the past, inefficient, or marginal to productivity.⁷ The prevailing paradigms for moderniza-

⁵ Landes, *The Unbound Prometheus: Technological Change, 1750 to the Present* (Cambridge, 1969), 99; Mantoux, *The Industrial Revolution in the Eighteenth Century: An Outline of the Beginnings of the Modern Factory System in England* (London, 1928); and Ashton, *The Industrial Revolution, 1760–1830* (Oxford, 1948). For both the criticism of the take-off and Rostow's reply, which reaffirms the early role of steam power, see his *The Stages of Economic Growth: A Non-Communist Manifesto* (2d edn., Cambridge, 1971). Also see his more recent conclusions in *How It All Began: Origins of the Modern Economy* (London, 1975), 166–67. John Lord's observation that the steam engine did not make "such a transformation in industry as is often imagined" seems to have been all but forgotten for analytic purposes; see Lord, *Capital and Steam Power, 1750–1800* (2d edn., London, 1966), 221.

⁶ Simon Kuznets, *Modern Economic Growth: Rate, Structure, and Spread* (New Haven, 1966); Asa Briggs, "Technology and Economic Development," in *Scientific Technology and Social Change: Readings from Scientific American* (San Francisco, 1974), 93–94; Carlo Cipolla, *The Economic History of World Population* (Baltimore, 1962), 35–36; and J. Frederick Dewhurst et al., *America's Needs and Resources: A New Survey* (New York, 1955), 902–05, 943. For a seminal critique of differing views of technology, see George H. Daniels, "The Big Questions in the History of American Technology," in H. J. Bass, ed., *The State of American History* (Chicago, 1970), 197–219.

⁷ See the critical analysis in Editorial Collective, "British Economic History and the Question of Work," *History Workshop*, no. 3 (1977): 1–4.

tion specify “improved” energy production and require replacement for all of the components of energy systems. Ashton concluded that “new forms of power” and “new transmitting mechanism[s],” in doing “work previously done by hand and muscle,” provided the essential “pivot on which industry swung into the modern age.”⁸ Similarly, in specifying the “improvements” that “constitute the Industrial Revolution,” Landes identified a chain of interrelated substitutions in energy systems that are generally accepted as the basis for modern industrialism: he pointed to the simultaneous “substitution of machines for human skills and effort; the substitution of inanimate for animate sources of power; . . . the substitution of mineral for vegetable or animal substances.”⁹

Over the past decade, as continuing crises have forced us to recognize the profound global impact of energy utilization, we have looked with renewed interest at the power patterns of the eighteenth and nineteenth centuries. The possible end of the fossil fuel era in industrial nations raised the specter of limited growth and encouraged the kind of questioning that has led historians to discover how very little information underlay orthodox propositions about the role of steam. We had come to credit observations such as those in the 1880 U.S. Census report that English industrial development reflected a “long reliance primarily on steam power,”¹⁰ although the era from 1800 to 1860 deserves to be called “the dark ages of the stationary steam engine.”¹¹ For all of our certitude that the “arrival of steam power . . . herald[ed] a new era,” until the 1970s few scholars had attempted to investigate the growth or spread of steam.¹² Despite assertions that steam served as the driving force for structural change, we knew quite little about the diffusion of this technology within England or about its transfer to the Continent or across the Atlantic. Certainly, until the research by Jeremy Attack, Fred Bateman, and Thomas Weiss, published as recently as 1980, regional “examination of its adoption in the United States” was, as the authors put it, “noticeably lacking.”¹³ Possessing at best a most imperfect account of the timing and diffusion of energy technology, we knew even less about steam consumption relative to other sources of motive power. In addition to these gaps in information, we had no quantitative evidence at all, for any country, of the actual contribution of different power sources to economic

⁸ Ashton, *The Industrial Revolution, 1760–1830*, 58.

⁹ Landes, *The Unbound Prometheus*, 41.

¹⁰ Hunter, *A History of Industrial Power in the United States*, 190–91. Although noting the importance of waterpower in England, Hunter still held to the traditional orthodoxy of steam’s significance there and thus emphasized how “markedly different” the power foundations were in the United States; *ibid.*, 159.

¹¹ For a challenging study that offers new insights and information, see G. N. von Tunzelmann, *Steam Power and British Industrialization to 1860* (Oxford, N.Y., 1978), 8.

¹² For comment on the inadequacy of previous data, see Jennifer Tann and M. J. Breckin, “The International Diffusion of the Watt Engine, 1775–1825,” *EcHR*, 2d ser., 31 (1978): 541. Other recent studies of diffusion include Eric H. Robinson, “The Early Diffusion of Steam Power,” *JEH*, 34 (1974): 91–107; John Kanefsky, “Motive Power in British Industry and the Accuracy of the 1870 Factory Return,” *EcHR*, 2d ser., 32 (1979): 360–75; Kanefsky and John Robey, “Steam Engines in Eighteenth-Century Britain: A Quantitative Assessment,” *Technology and Culture*, 21 (1980): 161–86; and von Tunzelmann, *Steam Power and British Industrialization to 1860*.

¹³ Attack *et al.*, “The Regional Diffusion and Adoption of the Steam Engine in American Manufacturing,” *JEH*, 40 (1980): 281–308. Also see Carroll W. Pursell, Jr., *Early Stationary Steam Engines in America: A Study in the Migration of a Technology* (Washington, 1969); and Temin, “Steam and Waterpower in the Early Nineteenth Century.”

growth, nor could we point to any efforts to relate energy to the composition of production and the structure of industry.¹⁴

On closer examination, it now appears that steam was not nearly as significant as has been supposed compared to other sources of power for the years 1780 to 1850. By 1800, even by 1850, hardly any movement to power-driven mechanization had yet begun in the vast majority of manufactures.¹⁵ And, even where machine methods did prevail, power often continued to be derived primarily from humans, horses, and waterwheels. We have never had a statistically valid basis for assuming that the rapid rise in English productivity from the last quarter of the eighteenth century was either dependent on, or decisively based on, steam, and we now have growing indications of the delayed internal diffusion and limited application of steam technology in both nations until at least the 1860s.

If at this point the accumulating evidence serves to qualify rather than to reinforce the idea that widespread reliance on steam-powered mechanization was necessary to propel the Industrial Revolution in either England or the United States, it is also becoming apparent that we must define a more adequate "energetic" context for probing the relative impact of different types of power on economic development as societies emerge from preindustrial, handicraft origins. To that end, we need a much clearer understanding of the energy flow relationships linking biological power sources, labor force characteristics, mechanization, and the processes of economic growth.

TO BEGIN WITH, instead of disregarding the proportion of muscle power consumed in these industrializing societies, we need to measure the work derived from humans and animals in different occupations and ascertain the input of biological power to industrial production. People and animals had been the chief source of motive power since the earliest epochs of human history. In terms of energy flow analysis, the trend to accelerating mechanization during the eighteenth century did not bring, as had been feared, their replacement. Quite the contrary, machine production re-emphasized the importance of animate power. The data available for the United States show that animate sources continued to supply more than half of its growing total horsepower until 1880.¹⁶

¹⁴ John Kanefsky recently noted, "Neither Professor Musson nor I have made any attempt to assess how far power capacity or usage is a satisfactory indicator of economic growth or development"; "Motive Power in British Industry," 375. Von Tunzelmann has raised the question of the backward linkages of steam to the iron industry, but he rejected the idea of mutually supportive growth at least up to 1850 (he has agreed with C. K. Hyde's refutation of forward linkages from steam to iron smelting); *Steam Power and British Industrialization to 1860*, 98–115. See Hyde, *Technological Change and the British Iron Industry, 1770–1870* (Princeton, 1977). There are no studies of energy and its impact on growth for the United States for the period prior to 1850, but for the century thereafter data have been developed to show the interrelationships between energy consumption and economic growth; see Sam H. Schurr and Bruce C. Netschert, *Energy in the American Economy, 1850–1975: An Economic Study of Its History and Prospects*, Resources for the Future (Baltimore, 1960). For an exemplary analysis relating energy to structural change, the composition of national output, and the efficiency of energy use (all of which are critical to appreciating energy's relationship to economic growth), see Joel Darmstadter *et al.*, *How Industrial Societies Use Energy: A Comparative Analysis* (Baltimore, 1977).

¹⁵ Musson, "Industrial Motive Power in the United Kingdom," 416, 436; C. K. Harley, "Skilled Labour and the Choice of Technique in Edwardian Industry," *Explorations in Economic History* [hereafter, *EEH*], 11 (1973–74): 391–414; and Raphael Samuel, "The Workshop of the World: Steam Power and Hand Technology in Mid-Victorian Britain," *History Workshop*, no. 3 (1977): 6–72.

¹⁶ Greenberg, "Energy Flow in a Changing Economy, 1815–1880," 34–38.

We sometimes forget that all of the original innovations ushering in the movement from handicraft to mechanization had been developed for muscle power. Although one machine performed the work of many identical tools, thereby creating staggering possibilities for production, it had to be set in motion by an outside force. In textile expansion, where technological breakthroughs were so critical, the early carding machines were turned by hand or driven by horses. The first machines for spinning weft were hand-powered jennies. Kay's flying shuttle, adopted in the 1760s, helped hand weavers operate wider looms more efficiently, while Clauser's circular loom, weaving 96,000 picks per minute, was worked by a single laborer. And Arkwright originally devised his spinning machine for horse power, although—after he had experimented with it, adding and removing horses to ascertain the power required per spindle, and after horse feed prices rose in the 1790s—he quickly adapted it to a more reliable, direct, and steady as well as stronger source of power, water.¹⁷

The impact of the move to mechanization was to redefine, rather than to replace, biological contributions in systems of energy transmission. As Arkwright's decision to install waterwheels revealed, to the degree that machines demanded uniform and continuous motion, they created new criteria for performance that accentuated the limitations and inherent power inadequacies of people and animals. Clearly, the larger the machines, and the greater the number that had to be turned simultaneously, the more continuous the force required. Under such circumstances, the deficiencies of animate power were more than obvious and frequently noted. Even a horse, as Marx observed in perfect seriousness, had a head of its own, and, quite apart from their expense, horse-teams could not be readily integrated into factory organization. People, unlike waterwheels, could not be counted on for sufficiently forceful or regular movement, and, unlike machines that powered multiple tools when set in motion, people had only two arms and two legs to work with.¹⁸

Paradoxically, as people and animals came to be measured by their ability to match machine operations, the very problems associated with the limited physical abilities of workers and animals probably stimulated an increase in animate power

¹⁷ Richard L. Hills, *Power in the Industrial Revolution* (Manchester, 1970), 89–207; and Jennifer Tann, "Richard Arkwright and Technology," *History*, 58 (1973): 29–44. Like Arkwright, James Watt in 1783 had calculated that a horse could work at a rate of 33,000 foot pounds per minute. In reality, a horse cannot do that much work continuously. The idea of a horsepower unit, however, came to be accepted, and the unit was standardized as the power needed for each horse to lift 33,000 pounds one foot high in a minute. The power of steam engines could then be measured in terms of steam horsepower just as the power of a waterwheel could be measured in terms of water horsepower.

¹⁸ Marx concluded that with modern productive techniques the limited physical abilities of workers would no longer dominate the allocation of capital or the organization of production. He recognized, however, that increased machine productivity did not displace the power of labor in terms of energy flows. Marx, *Capital*, 405–30. For an analysis of the subtlety of Marx's treatment of the links among science, technology, and economic growth, see Nathan Rosenberg, "Karl Marx on the Economic Role of Science," *Journal of Political Economy*, 82 (1974): 713–28. For the continued importance of animal power in England, see Musson, "Industrial Motive Power in the United Kingdom," 420; and, for the cost of animal power relative to steam, see von Tunzelmann, *Steam Power and British Industrialization to 1860*, 118–19. Nathan Rosenberg has noted the neglect of the importance of animal power in U.S. transportation and agriculture; *Technology and American Economic Growth*, 89.

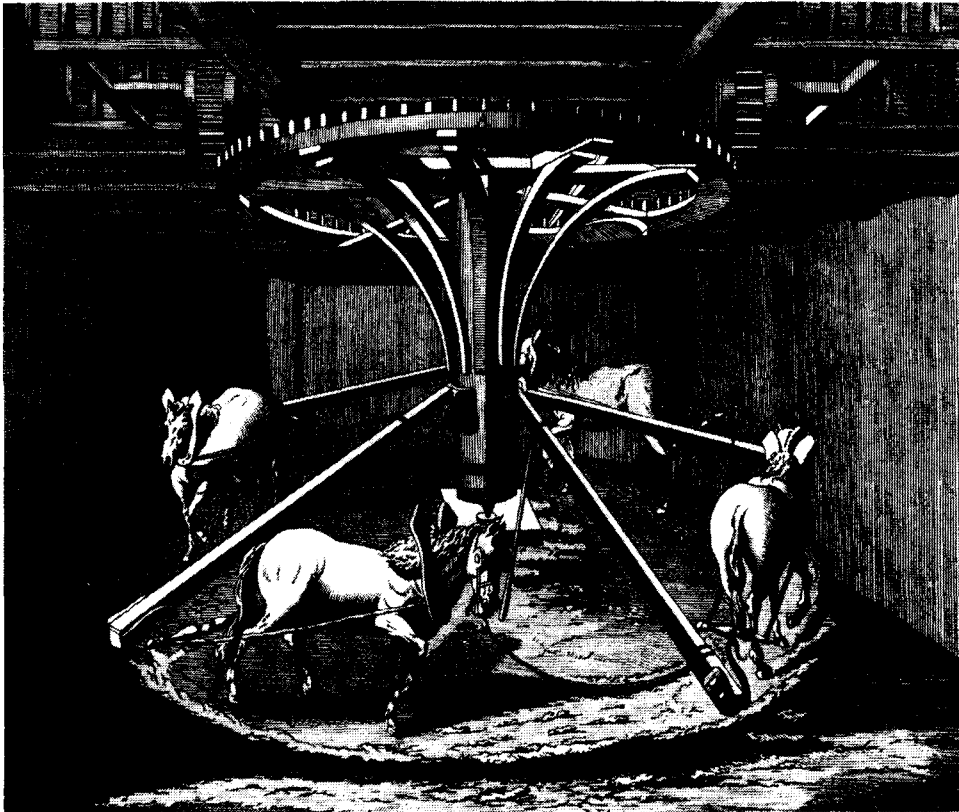


Figure 1: Horse engine. The horse engine was a major source of power and provided the model for “horsepower” as the unit to define power equivalencies. Reproduced courtesy of The Hagley Museum, Wilmington, Delaware.

supplies. As wind, water, and steam were introduced as the initial power sources for machines, a greater—not a lesser—absolute amount of biological motive power was probably called into use. What we designate as “labor-saving” machinery undoubtedly contributed to an immense expansion of goods per worker, but, instead of decreasing the power supplied by labor, the productiveness of modern methods increased the speed at which work was done and presumably resulted in greater work intensity.¹⁹ As Stephen A. Marglin has noted, the methods of labor supervision initiated with factory organization were specifically designed to increase work effort: “Disciplining the work force meant a larger output in return for a greater input of labor, not more output for the same input.”²⁰ Or, to convert this

¹⁹ According to Rosenberg, the introduction of interchangeable parts led to the elimination of time-consuming fitting by hand, and the worker only had to assemble a product; thus, “the new machines were labor-saving in that they simply eliminated the need for the highly labor-intensive stage of fitting.” *Technology and American Economic Growth*, 94–95. Certainly, assembly without handicraft fitting was “labor-saving” in the sense that more could be produced by each worker as hand methods gave way to machines and mass production techniques. But labor intensity for individual workers at these machines was not necessarily reduced.

²⁰ Marglin, “What Do Bosses Do?” in André Gorz, ed., *The Division of Labour: The Labour Process and Class Struggle in Modern Capitalism* (Atlantic Highlands, N.J., 1976), 36.

formulation into energetic terms, the number of machines tended, the force applied at different speeds and distances, and the hours worked in different types of physical activity all contribute to determining labor intensity—that is, the amount of energy expended.²¹

Unfortunately, historians for too long have neglected contemporary arguments, such as Lord Shaftesbury's in 1844, that, in noting the amount and rate of worker effort required by continuous motion techniques, pose questions relevant to modern energy analysis:

The labour performed by those engaged in the process of manufacture, is three times as great as in the beginning of such operations. Machinery has executed no doubt, the work that would demand the sinews of millions of men; but it has also prodigiously multiplied the labour of those who are governed by its fearful movements. . . . In 1815, the labour of following a pair of mules spinning cotton yarn of Nos. 40, reckoning 12 hours to the working-day, involved the necessity for walking 8 miles. . . . In 1832, the spinner put upon each mule 2,200 stretches, making a total of 4,400. In 1844, . . . 2,400 stretches, on each mule, making a total of 4,800 . . . , and in some cases, the amount of labour required is even still greater. . . . In the carding-room . . . one person there does the work formerly divided between the two. In the weaving-room, where a vast number of persons are employed, and principally females, . . . the labour has increased within the last few years, fully 10 per-cent., owing to the increased speed of the machinery in spinning.²²

Like other nineteenth-century observers of working conditions, Shaftesbury was invoking major principles of the soon-to-be-formulated theory of thermodynamics: first, that work is the application of a force through distance, and, second, that the physical effort required in industrial occupations is subject to a system of grading. Similarly, his assumption that biological and nonbiological sources supply equivalencies of power was supported by the discovery that exactly the same process occurs whether people, animals, or engines were converting the energy in fuel—that is, food or feed, wood or coal—into useful work and motive power.

Although nineteenth-century investigations of the physics of power identified the animate with the inanimate by establishing that both are subject to the same natural laws, the pervasive fear—expressed by Ruskin and Marx—that craftsmen and laborers were being reduced by modern methods to merely a source of power, that workers were becoming “merely the motive power of an implement machine,”²³ proved somewhat premature. As we are coming to recognize, the

²¹ For a study of how the human body uses energy and a discussion of the measurement methods for determining energy requirements for a variety of work activities, see J. V. G. A. Durnin and R. Passmore, *Energy, Work, and Leisure* (London, 1967). (Durnin and Passmore have also included examples of occupational studies in several countries.) In addition to establishing how much nutrition is needed for the actual work to be accomplished by animate exertion, the key aim is to find the actual energy expenditure. Three different but satisfactory methods have been devised for such estimates that can be prepared on a nationwide basis; see Vaclav Smil, “Energy Flows in the Developing World,” *American Scientist*, 67 (1979): 522–31. For other studies of energy budgeting, see Gerald Leach, *Energy and Food Production* (Guildford, England, 1976). At issue is the ratio of energy output divided by energy input.

²² Anthony Ashley, Lord Shaftesbury, Speech on Clause 2 of the Factories Bill, March 15, 1844, *Hansard's Parliamentary Debates*, 3d ser., 73: 1077–78, 1080. Also see Marx, *Capital*, 430–51.

²³ Marx, *Capital*, 410. Also see John Ruskin, *The Stones of Venice*, 2 (London, 1852): esp. chap. 6: “The Nature of Gothic.” In the 1840s, Julius Mayer established a scientific argument for the law of the conservation of energy that applied to animate as well as inanimate objects. By the 1890s, Cornell professor Robert A.

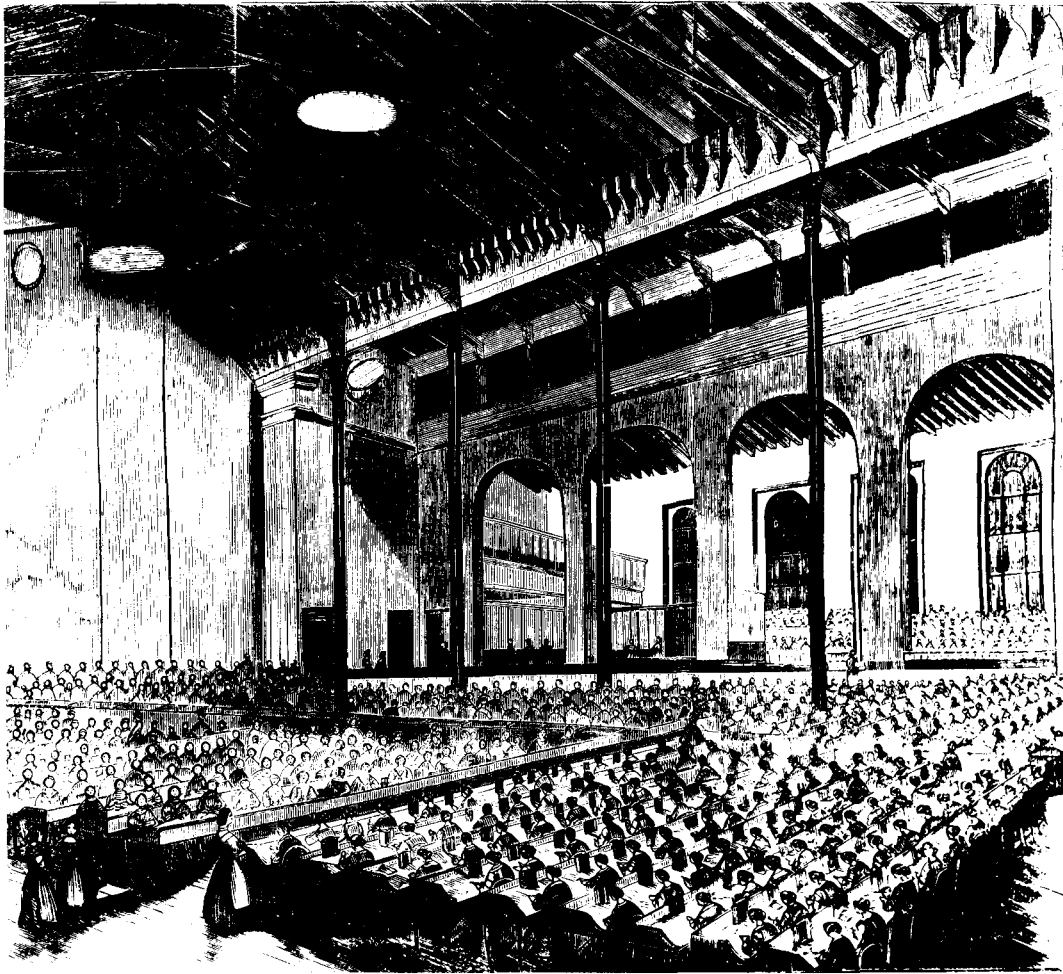


Figure 2: Cope & Company's Cigar Factory, Liverpool. This depiction of English women engaged as factory workers is typical of large-scale organization of manual workers in the mid-nineteenth century. Reproduced from *Harper's Weekly*, August 24, 1858, p. 525.

traditional skills of tool-held, handcraft methods continued to characterize manufacturing until at least the last quarter of the nineteenth century. Solutions to problems of power transmission for machines often came slowly, and just as often machines were themselves rejected as the appropriate technology for production. Contrary to conventional views, new research has shown both an extensive and extended reliance on handicrafts during much of the century. A. E. Musson's argument that "most manufacturing operations" in England "remained largely unmechanized until after 1870" has been confirmed by Raphael Samuel, who has demonstrated that nonmechanized methods persisted in all major sectors of agriculture, mining, and manufacture. In place of the accepted interpretation that the superiority of machines steadily broke down resistance, Samuel's thorough

Thurston, drawing on European scholarship, elaborated the current idea of the "animal as a machine"; Thurston, *Animal as a Machine and Prime Mover* (New York, 1894), 1–46. For a modern explanation of the efficiency of people and horses as machines, see Max Kleiber, *The Fire of Life: An Introduction to Animal Energetics* (New York, 1961).

examination shows that resistance to mechanization continued "in a whole spectrum of occupations" into the 1890s. In addition, he has called attention to an increase in the number of hand-powered steps, for a growing number of workers, wherever machine techniques were adopted.²⁴

Indeed, in those pursuits where mechanization was acclaimed for its greatest gains, the shift from hand methods occurred irregularly and became significant only in selective stages of production. In the rapidly growing and innovative cotton industry, for example, machine techniques remained confined, as late as 1810, to the preparatory stages of carding fibers and to spinning yarn. Efforts to mechanize fabric weaving lagged from the time of Cartwright's first loom in 1787 until the 1820s, first because of technical problems and then because of the hand-loom weavers, who took lower and lower rates to remain competitive. And, after power-driven weaving mills were set up in the 1830s, hand-loom operators still could not be displaced, for only they could make finer figured cotton cloth. In the once preeminent woolen industry, handicraft methods prevailed even longer. Although the preparatory processes of carding, sorting, scribbling, and slubbing were mechanized by 1800, technical obstacles emerged in spinning the fiber into yarn. It proved too difficult to devise a machine for all-purpose wool yarn, and Arkwright's water-frame, so well suited to cotton, did not make strong warps for woolens. Water-powered mules were not much in use before 1820 for spinning worsted, and, as late as 1850, both hand spinning of ordinary woolen yarn and hand-loom weaving of cloth dominated manufacturing.²⁵

Outside the cotton industry, the large-scale factory using new, heavy, power-driven machinery remained the exception before 1850.²⁶ In fact, the steps through cotton and worsted weaving, and the preparatory steps in metal working, were the only major operations that were thoroughly mechanized in any key English industry by mid-century. Finishing operations in textiles and in the most advanced of British manufactures continued to be done by hand. By 1850, the major exports to Western Europe, from the nation that pioneered mechanization, consisted of semifinished output, such as iron billets, bars, girders, and cotton and worsted yarn, all of which was then turned into finished products by handicraft workers abroad.²⁷ Indeed, small-scale workshops employing handicraft techniques still dominated in major industries into the Edwardian period, partly because of consumer preference for handmade goods, but primarily because of the abundance of skilled labor at cheaper rates than anywhere else in the world. As C. K. Harley has stressed, rather than indicating any failure in English entrepreneurial behavior, this persistence of handicrafts reveals how important labor force skill characteristics proved in the rational choice of production methods.²⁸

²⁴ Musson, "Industrial Motive Power in the United Kingdom," 416; and Samuel, "The Workshop of the World," 6–72.

²⁵ Von Tunzelmann, *Steam Power and British Industrialization to 1860*, 182–87, 195; Lane, *The Industrial Revolution*, 179–80; Ralph Davis, *The Industrial Revolution and British Overseas Trade* (Leicester, 1979), 14–21; and Hills, *Power in the Industrial Revolution*, 15–18, 191–247.

²⁶ Harley, "Skilled Labour and the Choice of Technique," 391–414; Musson, "Industrial Motive Power in the United Kingdom," 435; and Phyllis Deane, *The First Industrial Revolution* (Cambridge, 1965), 273.

²⁷ Davis, *The Industrial Revolution and British Overseas Trade*, 33–34.

²⁸ Harley, "Skilled Labour and the Choice of Technique," 391–414.

If the relative abundance of skilled labor largely explains Britain's delayed move to mechanization, the relative scarcity of skilled labor in America may account for the early design of simplified production processes and self-acting machinery, which were especially suited to the nature of the labor force.²⁹ The dramatic growth rate in U.S. manufacturing, a 59 percent increase per decade between 1809 and 1840, had been accompanied by the gradual adoption of interchangeable parts, assembly lines, integration of productive processes, and the creation of a machine tool industry. Despite the prevalence of handicrafts at mid-century, the productivity of continuous flow technology so surpassed British practices that the "American System" stood as a challenge to the greatest industrial economy in the world.³⁰

What American entrepreneurs could count as labor-saving investment, when calculating labor and capital as relative costs of production, may not, however, have saved muscle power, if that is measured by the amount and rate of work input per worker. For men, women, and children, labor-saving machinery meant a disciplined six-day week, twelve-hour days, and a new regularity in the speed of work. Since New England textile manufacturers evidently ran their machines more rapidly than did their English counterparts to achieve greater output, we can infer even greater labor intensity.³¹ G. N. von Tunzelmann has suggested that the lower cost of water and steam power in the United States, not merely the higher real

²⁹ The connections between capital, the availability of skilled versus unskilled labor, and technological change in the United States and England have been the subject of considerable, unresolved controversy. One of the prevailing assumptions is that specialization in the United States was "labor-saving" in that it actually reduced human effort; since it also reduced the time worked on individual product units, it simultaneously cut labor costs for manufacturers. But, exactly how labor characteristics should be figured relative to capital demands a more complicated response than H. J. Habakkuk has provided, since so many substitutions were possible among fuels, power sources, types of machinery, and the amount of capital invested; see Habakkuk, *American and British Technology in the Nineteenth Century: The Search for Labour-Saving Inventions* (Cambridge, 1962). Also see Peter Temin, "Labor Scarcity and the Problems of American Industrial Efficiency in the 1850s," *JEH*, 26 (1966): 277–98; D. L. Brito and Jeffrey G. Williamson, "Skilled Labor and Nineteenth-Century Anglo-American Managerial Behavior," *EEH*, 10 (1972–73): 235–51; Paul Uselding and Bruce Juba, "Biased Technical Progress in American Manufacturing, 1839–99," *ibid.*, 11 (1973–74): 55–72; Rosenberg, *Technology and American Economic Growth*, 92–95; and especially Paul A. David, "Labor Scarcity and the Problem of Technological Practice and Progress in Nineteenth-Century America," in his *Technological Choice, Innovation, and Economic Growth: Essays on American and British Experience in the Nineteenth Century* (Cambridge, 1975), 19–91.

³⁰ Thomas C. Cochran has argued that Britain's industrial leadership has been exaggerated and that by 1812 the "critical point" for Anglo-American industrial development had been reached; Cochran, "An Analytical View of Early American Business and Industry," in Joseph R. Frese and Jacob Judd, eds., *Business Enterprise in Early New York* (Tarrytown, N.Y., 1979), 1–13. Also see Thomas C. Cochran, *Frontiers of Change: Early Industrialization* (New York, 1981), 50–115; and Nathan Rosenberg, ed., *The American System of Manufactures* (Edinburgh, 1969).

³¹ David J. Jeremy, "Innovation in American Textile Technology during the Early Nineteenth Century," *Technology and Culture*, 14 (1973): 47–52. Jeremy has noted the higher operating speeds, yet he supposed female operators—even those weavers who worked longer hours at piece rates—and children worked less hard and used less energy. He has included, moreover, high operating speeds as one of the eight labor-saving modifications of British technology that the Americans introduced, and he has also called attention to the increased size of machines, which were operated without additional workers, as "the major labor-saving economy of scale." The question is whether the increase in inanimate power to which Jeremy pointed actually lessened the power input of workers who had to tend larger continuous-motion spinning machines with speeds of up to 7,000 rpm. Also see David J. Jeremy, *The Transatlantic Industrial Revolution: The Diffusion of Textile Technologies between Britain and America, 1790–1830s* (Cambridge, Mass., 1981). For a discussion of labor intensity in Anglo-American manual mule spinning during the last half of the nineteenth century, which points to machine speed-up, yarn breakage, and cotton yarn quality, see William H. Lazonick, "Production Relations, Labor Productivity, and Choice of Technique: British and U.S. Cotton Spinning," *JEH*, 41 (1981): 491–516.

wages of workers, accounts for the faster spindle speeds—that cheaper power “dictated” different, more power-intensive technology run at higher speeds.³² Unquestionably, American workers derived greater productivity per spindle; at such speeds, however, yarn broke with greater frequency, which necessitated still additional labor for repairs. An unresolved question, then, is exactly how higher speeds of machine operation affected the power production of men, women, and children. The answer should help us understand more fully the relationship between power consumption, relative costs, and industrial production.

Without a measure of the power derived from biological sources, we have no guide to absolute and relative levels of energy consumption and, therefore, no viable framework for economic and energy analysis. The problem is further complicated by the selective figures available, which reveal that people and animals were the major converters of fuel into work and the most important sources of power in the United States in 1850, and even as late as 1880.³³ Despite the impressiveness of these data in underscoring the prolonged primacy of muscle power, no similar estimates exist for either nation for the period prior to 1850. And no data exist for a sector comparison, although it has been suggested that “even in the [English] cotton industry it is likely that human beings supplied more motive power than steam-engines up to the 1820s.”³⁴ Not only would information on biological power testify to these relative contributions, but measurements of a range of energy inputs for different productive activities may also undermine longstanding premises that mechanization invariably reduces workers’ effort, that mineral-fueled technology is more energy efficient than handicrafts, or that industrialization always involves the substitution of inanimate for animate sources of power.³⁵ Certainly the proliferation and persistence of handicrafts should alter our perception of the constituents of social change, just as indications of significant labor-intensiveness should prompt closer attention to allocations of capital and labor in facilitating economic growth.

ALTHOUGH DATA ARE STILL TO BE AMASSED on biological energy sources, current reappraisals emphasizing Anglo-American reliance on waterpower have already challenged prevalent supply-side power premises. Implicit, sometimes quite explicit, in the standard literature is the argument that an energy shortage threatened the take-off of the Industrial Revolution. Presumably, available supplies of wood and

³² For the relationship of speed to inanimate power costs, see von Tunzelmann, *Steam Power and British Industrialization to 1860*, 267–76.

³³ Greenberg, “Energy Flow in a Changing Economy, 1815–1880,” 34–38; and Dewhurst *et al.*, *America’s Needs and Resources*, 1113–16.

³⁴ Von Tunzelmann, *Steam Power and British Industrialization to 1860*, 295.

³⁵ For further discussion of energy budgeting procedures and results, see Paul Ehrlich *et al.*, *Ecomoscience: Population, Resources, and Environment* (San Francisco, 1977); Howard T. Odum, *Environment, Power, and Society* (New York, 1971); Roy A. Rappaport, “The Flow of Energy in an Agricultural Society,” *Scientific American*, September 1971, pp. 117–32; Daniel Gross and Barbara Underwood, “Technological Change and Caloric Costs: Sisal Agriculture in Northeastern Brazil,” *American Anthropologist*, 73 (1971): 725–39; Leach, *Energy and Food Production*; and David Pimentel, “Energy and Agriculture,” in Margaret R. Biswas and Asit K. Biswas, eds., *Food, Climate, and Man* (New York, 1979), 73–106.

water, wind, and muscle power were running out, or had reached their limit, so that traditional sources could not have supported further economic growth. According to this reading, the incipient crisis was averted only by the contribution of new power technology. In the words of Phyllis Deane,

The most crucial and general of the bottlenecks limiting the expansion of the British economy on the eve of the British industrial revolution (that is, in the middle of the eighteenth century) were two: they were the shortage of wood and the shortage of power. . . .

If we were to try to single out the crucial inventions which made the industrial revolution possible and ensured a continuous process of industrialization and technical change, and hence sustained economic growth, it seems that the choice would fall on the steam-engine on the one hand and on the other Cort's puddling process, which made a cheap and acceptable British malleable iron.³⁶

There are three highly questionable suppositions operative here: first, that an energy scarcity had set in; second, that the invention of the steam engine, a device for converting some form of fuel into work, remedied the power deficiency; and, third, that a leveling off or decrease in specific fuels or power supplies necessarily creates a barrier to growth.³⁷

No one has yet examined how English economic growth was affected by the rate, efficiency, or distribution of energy use for this period, so that the third supposition is unwarranted, pending investigation. And the first two do not survive careful econometric scrutiny. Von Tunzelmann's findings give support to Musson's contention that "continued and increasing use of waterpower in the Industrial Revolution has been comparatively neglected," overshadowed by undue emphasis on Watt's invention.³⁸ Moreover, as John Kanefsky's recent evaluation of the English *Factory Report* of 1870 shows, "waterpower continued to provide the lion's share of Britain's fixed power requirements long into the nineteenth century,"³⁹ whatever the relationship between increasing rates of steam consumption and the gross national product.

To these perceptions of Britain's past power trends, a similar account for the United States can be added. Louis Hunter has documented the striking contribution of waterpower to American development over two and a half centuries of economic and social change. His encompassing overview, Anthony F. C. Wallace's exploration of Rockdale, and a number of new social histories of nineteenth-century mill towns demonstrate the socioeconomic significance of waterpower in the U.S. transition to mature industrialism.⁴⁰

³⁶ Deane, *The First Industrial Revolution*, 129–30.

³⁷ Analysis of long-term trends from 1850 reveals that energy use and the GNP have not necessarily corresponded, rising and falling in tandem. Moreover, energy consumption per capita in the United States was higher in relation to the GNP in 1880 than it was in 1950; Greenberg, "Energy Flow in a Changing Economy, 1815–1880," 38–46.

³⁸ Musson, "Industrial Motive Power in the United Kingdom," 416–20; and von Tunzelmann, *Steam Power and British Industrialization to 1860*, 1–14, 283–89.

³⁹ Kanefsky, "Motive Power in British Industry," 368–72; and Kanefsky and Robey, "Steam Engines in Eighteenth-Century Britain," 186.

⁴⁰ Louis Hunter, "Waterpower in the Century of the Steam Engine," in Brooke Hindle, ed., *America's Wooden*

Waterpower had been integrated into the economy and culture of rural America until the late nineteenth century, when a highly complex and extensive interchange among people, goods, and capital finally altered market conditions for competing energy sources. The disadvantages of a power technology fixed by geography, determined by terrain, and subject to seasonal effects on stream flow were outweighed during most of the century by the ability of waterpower to satisfy the demands of diverse energy users in the developing nation. Watermills, easily constructed with local timber and stone, served semisubsistence frontier communities at the same time that they directly powered larger processing and manufacturing operations for rural districts imperfectly linked to distant commercial centers. When located on good sites, with strong flows and good heads, breastwheels could also meet the power demands of heavily financed, large-scale New England factories that were economically tied to world cotton markets.

As of 1820, there were one hundred waterwheels for every steam engine, a statistic that forcefully illustrates the importance of fixed power supplies in the expanding economy. Into the 1850s manufacturers still preferred waterwheels or the newly introduced and more efficient turbines to steam engines. The largest textile manufacturing enterprise of its time, Pacific Mills, capitalized at \$1.2 million, opened in 1853 powered by water. And by 1870 direct falling water still accounted for almost half of all installed horsepower in manufacturing, while waterwheels still outnumbered engines.⁴¹

Instead of taking divergent energy paths, England and the United States made remarkably similar choices in the use of waterpower. In both nations custom and long-term water rights were of central importance in decision-making. In addition to these criteria, which could impede change to new power technology, the low marginal cost of water, coupled to its performance when compared with steam engineering, made it the best of possible alternatives in power-intensive operations. Consequently, even in English cotton manufacturing, where steam achieved its most stunning success, water continued to be an important source of power for weaving and finishing in the continually growing industry. More automated equipment would have consumed relatively larger shares of power so that, compared with water costs, declining steam costs would have had to fall more rapidly than they in fact did to spur replacement and changeover.⁴²

It seems safe to say that waterpower figured as the major determinant of industrial location through the first half of the nineteenth century and that

Age: Aspects of Its Early Technology (Tarrytown, N.Y., 1975), 160–92, and *A History of Industrial Power*; Wallace, *Rockdale: The Growth of an American Village in the Early Industrial Revolution* (New York, 1970; reprint edn., 1980); Susan E. Hirsh, *Roots of the American Working Class: The Industrialization of Crafts in Newark, 1800–1860* (Philadelphia, 1978); Carl Siracusa, *A Mechanical People: Perceptions of the Industrial Order in Massachusetts, 1815–1880* (Middletown, Conn., 1979); and Peter J. Coleman, *The Transformation of Rhode Island, 1790–1860* (Providence, 1963).

⁴¹ Edwin T. Layton, Jr., "Scientific Technology, 1845–1900: The Hydraulic Turbine and the Origins of American Industrial Research," *Technology and Culture*, 20 (1979): 66–70, 82; Atack *et al.*, "Regional Diffusion and Adoption of the Steam Engine," 281–82, 289; and Hunter, *A History of Industrial Power*, 503.

⁴² Atack *et al.*, "Regional Diffusion and Adoption of the Steam Engine," 288–300, esp. 294–99; Hunter, *A History of Industrial Power*, 181, 508, 516–28; and von Tunzelmann, *Steam Power and British Industrialization to 1860*, 286–92.

technical change, based on the increasing utilization of waterpower capacity, transformed the scale of production and the level of industrial output in both nations far more than the introduction of steam did.

THIS PATTERN OF RELIANCE on known energy systems is completely at odds with the generally accepted principle that industrialization is a process necessarily tied to the widespread use of both new energy converters and new sources of energy and power.⁴³ Yet the expansion of known and tried power supplies stands in clear contrast to the restricted industrial diffusion of steam technology and the minimal consumption of steam power in Anglo-American manufacturing by mid-century.

From the introduction of Thomas Newcomen's atmospheric engine in England in 1712, and through the rest of the eighteenth century, this new energy technology remained concentrated outside manufacturing wherever water pumping operations required more energy than men and horses could supply. Evidently, the atmospheric model, designed for pumping water out of coal mines, was built in even greater numbers than we originally thought. And, similarly, until recently we underestimated the horsepower available from Watt's improved design, which replaced the reciprocating motion of a steam-driven piston with the rotary motion of a wheel for directly powering machinery. Nevertheless, according to the latest data, in 1800, eighty-five years after the atmospheric engine had been introduced and some twenty years after the introduction of rotary construction, almost 50 percent of all engines were still located in mining, and some 40 percent of those were used as water pumps in deep mine shafts plagued by flooding. Town waterworks and canals employed another 4 percent for pumping, while most of the 12 percent located in metal works were also used to pump water. Even among the 21 percent used in the growing textile industry and the 5 percent used in food processing, most functioned as an accessory form of pumping power for water-wheels.⁴⁴

By all standards, "steam was still in its infancy" by 1800.⁴⁵ Although John Kanefsky and John Robey have documented that over twenty-one hundred

⁴³ In addition to works cited earlier (such as Mumford's *Technics and Civilization*, Dewhurst et al.'s *America's Needs and Resources*, Cipolla's *The Economic History of World Population*, Landes's *The Unbound Prometheus*, and Cottrell's *Energy and Society*), see E. R. Service, *Cultural Evolutionism: Theory and Practice* (New York, 1971); Clifford Geertz, *Agricultural Involvement* (Berkeley and Los Angeles, 1963); Lynton Caldwell, "Energy and the Structure of Social Institutions," *Human Ecology*, 4 (1976): 31-44; and J. J. Poggie and R. N. Lynch, eds., *Rethinking Modernization: Anthropological Perspectives* (Westport, Conn., 1974), 353-75.

⁴⁴ Kanefsky and Robey, "Steam Engines in Eighteenth-Century Britain," 161-85, esp. 179-82. In Newcomen's engine, steam formed in a boiler, fired by either coal or wood, entered a cylinder at low pressure and was cooled by injections of cold water. As the steam condensed, forming a partial vacuum, a piston was pushed into the cylinder by atmospheric pressure and operated a pump-rod attached to a working beam. Newcomen's pumping engine was both slow and inefficient, wasting more than 99 percent of the energy in its fuel. Watt's major contribution was his introduction of a separate condenser cylinder that allowed the steam to expand more and do more work before being exhausted from the engine. Watt's best designed engine was more efficient, but it still had a thermal efficiency of only 5 percent. Earl Cook, *Man, Energy, and Society* (San Francisco, 1976), 30-31, 39. Also see Eugene Ferguson, "The Origins of the Steam Engine," *Scientific American*, January 1964, pp. 72-81; and Thomas Ewbank, *A Descriptive and Historical Account of Hydraulic and Other Machines for Raising Water* (1842; reprint edn., New York, 1972), 231-364, 453-74.

⁴⁵ Musson, "Industrial Motive Power in the United Kingdom," 416. Also see Kanefsky and Robey, "Steam Engines in Eighteenth-Century Britain," 161-86.

engines of all types had been built during the eighteenth century (and the final tabulation may reach twenty-five hundred), any estimates of the power available from these engines need to be approached with some caution, since not only did the engines frequently break down and need repairs but some served merely as occasional supplemental power sources. In other words, capacity multiplied by the number of engines built does not indicate the actual power provided. Indeed, only about fifteen hundred engines with an average capacity of some 15 horsepower were in actual operation in 1800. Estimates of the total horsepower they supplied range from 20,000 to 35,000. Steam for all uses, therefore, contributed only a fraction to the nation's total inanimate motive force of 170,000 horsepower, and it supplied much less of the direct force needed in manufacturing to set machines in motion. Unquestionably, new engine building and steam use accelerated toward the end of the eighteenth century, paralleling a wave of inventions needing unprecedented amounts of power.⁴⁶ Yet, wherever sufficient alternatives were available (whether wind, water, or muscle power), steam remained the choice of last resort for the majority of manufacturers.

Both technical and economic factors compounded manufacturers' resistance, which when coupled to entrepreneurial conservatism delayed more rapid diffusion. Contrary to the supposition that the convergence of machine methods and advances in power engineering after 1780 radically reduced costs and brought general adoption,⁴⁷ the reverse is true. The new, more complex, and heavier rotary designs proved expensive. To large capital outlays, manufacturers had to add constant repair fees. The possibility of high fuel prices in different regions added a further reason not to invest in what was often considered an inferior invention, unable to compete in terms of either cost or performance with the steady, smooth motion of waterwheels. Although rotary engines were used for a widening variety of industrial tasks by 1800, adoptions were almost entirely limited to special circumstances where water was scarce. And British manufacturers eschewed both Richard Trevithick's smaller, cheaper, high-pressure engine and the more fuel-efficient, but expensive, high-pressure, Cornish condensing engine until the 1840s.⁴⁸

As capital-intensive industrialism matured during the first half of the nineteenth century, steam consumption remained at a low level and engine distribution

⁴⁶ Kanefsky, "Motive Power in British Industry," 374–75; and J. R. Harris, "The Employment of Steam Power in the Eighteenth Century," *History*, 52 (1967): 133–48.

⁴⁷ Rostow, *How It All Began*, 166–67; and Rosenberg, *Technology and American Economic Growth*, 121.

⁴⁸ Hills, *Power in the Industrial Revolution*, chaps. 8–10; Kanefsky and Robey, "Steam Engines in Eighteenth-Century Britain," 182; Temin, "Steam and Waterpower in the Early Nineteenth Century," 188; and von Tunzelmann, *Steam Power and British Industrialization to 1860*, 21–22, 88–92, 288. In the nineteenth century, Cornish engines were thought of as "high pressure," and they have subsequently been referred to as "high-pressure engines." Since they actually operated at pressures under 100 pounds per square inch, a recent analysis groups them with the low-pressure Watt and Newcomen engines. See Harlan I. Halsey, "The Choice between High-Pressure and Low-Pressure Steam Power in America in the Early Nineteenth Century," *JEH*, 41 (1981): 723–44. Halsey's argument is that specific economic and technological factors determined regional engine choice in both the United States and England. In particular, Halsey has used fuel prices and interest rates to explain the differing distribution of engine types, concluding that the high-pressure model was cheaper under American conditions, if less fuel efficient, than the low-pressure type more generally used in England to the 1840s.

continued to be confined along a narrowly marked frontier. After the initial period when steam engines were introduced and adopted in industry, they then penetrated the market slowly and for quite specific uses—a pattern that is consistent with conclusions based on statistical analysis of later energy transitions.

First of all, a new primary energy, like any new technology, is introduced first by drawing capital and resources from the industrial and economic environment. This “investment in faith” usually shows up with very fast rates of market penetration right at the beginning followed by a reflection period, after which speed is resumed in compliance with the market. As a new technology, now a new industry, has to walk on its own legs, its speed of penetration is always lower.⁴⁹

Indeed, instead of driving the broad industrial advance ordinarily supposed, at the growth rates of the late eighteenth century, steam technology remained limited in diffusion and in application. As a power source, steam continued to be noticeably concentrated in a few power-intensive industries—textiles, coal, and the primary processes of iron production—and, even then, it was the sole supply in none. Steam, in fact, remained a relative rarity in British industry before mid-century. In 1850 British locomotives utilized 750,000 steam horsepower; that was probably one-half more than the entire manufacturing sector, even if manufacturing consumption is estimated as high as 500,000 steam horsepower.⁵⁰

It comes as something of a surprise to realize that in textiles, where steam is acclaimed for its extraordinary achievements, it was employed in only a few steps in production. In the cotton districts of Manchester and Lancashire, which went “steam mill mad,” steam was used mainly for the preparatory functions and for spinning yarn; it was not applied to weaving until the 1820s and was hardly used at all in finishing operations for fulling, dying, and bleaching even at mid-century. Paradoxically, too, contemporaries associated steam with small manufactures, since operations of any scale depended on a combination of steam, water, and muscle power.⁵¹

Whatever the controversies over when the massive growth in steam occurred

⁴⁹ C. Marchetti and N. Nakicenovic, *The Dynamics of Energy Systems and the Logistic Substitution Model*, Report of the International Institute for Applied Systems Analysis (Laxembourg, Austria, 1980), 6–7. Although the data used for this conclusion were taken from some three hundred cases in the period after 1850, the pattern derived from this logistic substitution analysis can be applied to the period from the 1780s to the 1850s. If the rates of growth of per capita input and output of energy are used to account for the varying energy efficiency of technology, the twenty-five years from 1855 to 1880 encompass the period of most rapid growth; Palmer C. Putnam, *Energy in the Future* (New York, 1953), 94–95, 90–93. Using data on power capacity of engines, which does not account for efficiency or actual production of power or for actual consumption of power, Kanefsky has estimated that the growth rate for steam power was faster from 1800 to 1870—roughly 80 percent per decade—and slowed after 1870—to 50 percent per decade; “Motive Power in British Industry,” 374–75.

⁵⁰ In 1850 locomotives used 750,000 “indicated” steam h.p., while the estimate for the industrial sector is 300,000 “nominal” steam h.p. These figures are not comparable, since indicated horsepower refers to a quantity of steam actually produced, while nominal horsepower does not gauge the real power supplied and takes no account of efficiencies (it is calculated from the physical size of the engines). The figure of 500,000 indicated steam h.p. in manufacturing is a calculation suggested by a referee of this essay. If 500,000 i.h.p. is a more likely figure, Musson may have understated the case for steam by using the 300,000 “nominal” h.p.; the larger argument about the delayed reliance on steam, however, remains unchanged. Musson, “Industrial Motive Power in the United Kingdom,” 435. Also see Robin M. Reeve, *The Industrial Revolution, 1750–1850* (London, 1971), 20; and Deane, *The First Industrial Revolution*, 273.

⁵¹ Davis, *The Industrial Revolution and British Overseas Trade*, 18–22; and Musson, “Industrial Motive Power in the United Kingdom,” 429–35. For a comment on wind power, see *ibid.*, 420.

(Musson has argued for the period from 1870 to 1914 and Kanefsky for an earlier and steadier growth rate), by 1850 steam consumption had made relatively minimal inroads in manufacturing. In terms of total steam consumption, moreover, the high ratios consumed in cotton manufacturing by the 1820s had foreshadowed a half-century trend of power distribution. As late as 1870 textiles alone accounted for almost half of all steam used in British manufacturing and an estimated one-third of total steam power capacity for the nation. The restricted allocation of the remainder in coal mining, iron smelting, forges, and foundries also reveals little divergence from earlier precedents.⁵² The use of steam in these important industries was certainly significant, but other major industries—chemicals, clothing, building, woodworking, shipbuilding, small metal works, and boots and shoes—made little use of this form of power.

England served as a model in these years, as it had in so many ways since the colonial era. Typically, however, Americans on the rapidly developing edge of Europe's western frontier had exercised great selectivity in choosing ideas or implements.⁵³ When it came to this new technology, its productive potential unquestionably generated great admiration. Even Thomas Jefferson, who was in the habit of carefully weighing the social costs and benefits of industrial changes, saw the positive power of steam for encouraging American self-sufficiency. Writing to a friend from England in 1786, he looked to the future dual use of steam in manufactures and transportation:

I could write you volumes on the improvements which I find made and making here in the arts. One deserves particular notice, because it is simple, great, and likely to have extensive consequences. It is the application of steam as an agent for working grist mills. . . . I hear you are applying this same agent in America to navigate boats, and I have little doubt but that it will be applied generally to machines, so as to supersede the use of water ponds, and of course to lay open all the streams for navigation. We know that steam is one of the most powerful engines we can employ; and in America fuel is abundant.⁵⁴

Because of its dramatic possibilities, "almost immediately steam became symbolic of the emergent industrial order."⁵⁵ But not even the support of a national rhetoric of progress could insure adoption. The disparity between the enthusiasm and the delay in industrial use suggests the degree to which energy innovation had to fit indigenous criteria of "appropriate technology" and mesh with perceived needs to overcome the constraints created by local resources, labor, capital supplies, and what Walter Bagehot called "the cake of custom."

Few barriers had arisen to prevent the transfer of this technology. Twice between 1715 and 1825, first in the case of the atmospheric engine and then with Boulton and Watt's rotary model, international dissemination of information had quickly followed invention. The sequence of diffusion evident in the United States also

⁵² Kanefsky, "Motive Power in British Industry," 373–75.

⁵³ Rosenberg, *Technology and American Economic Growth*, chaps. 3–4.

⁵⁴ Jefferson to Charles Thomson, April 22, 1786, in [Jefferson] *The Papers of Thomas Jefferson*, ed. Julian P. Boyd, 9 (Princeton, 1954): 400–01, as quoted in Kasson, *Civilizing the Machine*, 23. Also see Pursell, *Early Stationary Steam Engines*, 16.

⁵⁵ Kasson, *Civilizing the Machine*, 22–50.

recurred, though at a differing pace, in Germany, France, Russia, and Belgium: absorption of details; importation of engines and parts; an accretion of native engineering skills either through espionage, immigration, or technical aid; and then, the last step, production for home consumption. Yet a similar lag developed in all of these nations. Engines were wanted for few manufacturing purposes, and the primary demand for steam power remained restricted to mining and transportation.⁵⁶

If steam had offered a viable alternative, if demand had warranted, the technical foundations could certainly have supported more extensive usage. By 1730, not twenty years after its introduction in England, factories in Western Europe were already producing Newcomen engines. By the 1790s, mechanics and iron founders in Europe and in the United States had a long familiarity with all of the steps from casting hardware to constructing entire engines. With Boulton and Watt's commercialization campaign after 1775, information spread from Peru to St. Petersburg, and a formal network of sales personnel added technical insights on industrial capabilities to the ideas informally passed on by mechanics, businessmen, and scientists. Nevertheless, as in England, stationary engines, including rotary models, continued to be used into the nineteenth century largely to drive pumps in mining, in waterworks, in land draining, and, in conjunction with waterwheels, in milling.⁵⁷

By the first decade of the nineteenth century, a milieu of ongoing experimentation could be found on both sides of the Atlantic. So great was the preoccupation of competent mechanics with specific problems that they brought simultaneous solutions to major technical obstacles of fuel efficiency and portability. Their convergent concerns reveal a community of specialists in both nations who were working from established premises and who, by applying their new skills, led the way to finding new principles. Rather than being on-the-job tinkerers or theoretical scientists, these inventors belonged to a relatively new group of engineer-promoters who actively pursued their patent rights and tracked every opportunity to merchandize their devices.

In one of the earliest American efforts to apply steam technology, John Fitch—working exclusively from published sources—developed his drawings for a steamboat. Hoping for encouragement, if not a subsidy, he petitioned the Congress of the Confederation and the legislature of Virginia in 1785, the same year in which he submitted his plans to Benjamin Franklin and the American Philosophical Society. Two years later, Pennsylvania, New York, and Delaware gave Fitch a monopoly on the manufacture and operation of “all boats propelled by fire or steam within their jurisdiction.” Fitch initiated commercial marine transportation when he opened a regular service from Philadelphia to Wilmington, Trenton, and Chester in 1790.⁵⁸

⁵⁶ Tann and Breckin, “The International Diffusion of the Watt Engine,” 541–60.

⁵⁷ *Ibid.*; Jennifer Tann, “Marketing Methods in the International Steam Engine Market: The Case of Boulton and Watt,” *JEH*, 38 (1978): 363–91; and Pursell, *Early Stationary Steam Engines*, 4–10.

⁵⁸ Thomas Boyd, *Poor John Fitch, Inventor of the Steamboat* (1935; reprint edn., Freeport, N.Y., 1971), 131–302.

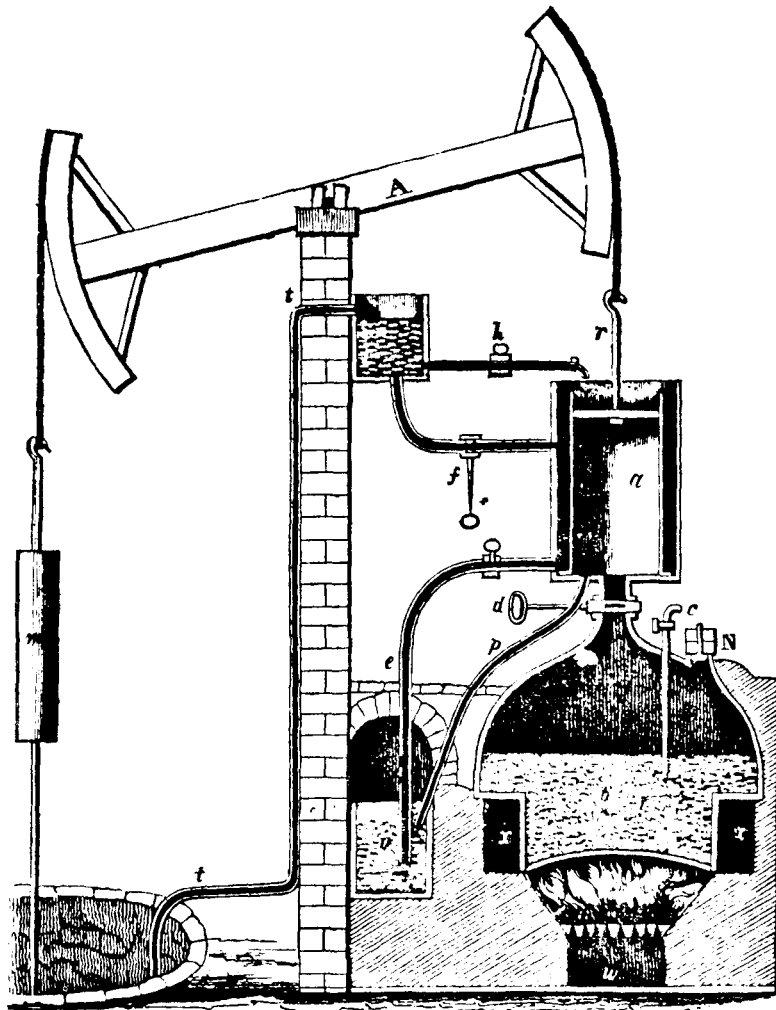


Figure 3: Newcomen and Cawley's first atmospheric engine of 1705, improved in 1712. The main pump rod (m) is suspended from the vibrating beam (A). The steam generated by the fire under the boiler fills the cylinder (a). When cold water enters from the pipe (f), vapors condense, forming a vacuum under the piston (r). Atmospheric pressure pushing down the piston raises the opposite end of the beam, the pump rod, and water from the mines. Reproduced from Thomas Ewbank, *A Descriptive and Historical Account of Hydraulic and Other Machines for Raising Water* (London, 1842), 465.

Meanwhile, Oliver Evans, an inventor-millwright, had turned his attention from the automation of flour milling using waterpower to the stationary engine, looking to solve the problem of low fuel efficiency. Evans wondered why steam had not been applied as a power source in the atmospheric engine, but only to create a vacuum to press down a piston. He was at work on a high-pressure boiler so that steam pressure would directly drive machines when Trevithick patented an almost identical machine in England. By 1803 Evans had introduced his facsimile in the United States, and he made a complete set of his engineering plans publicly available in 1805, when he published *The Abortion of the Young Steam Engineer's*

Guide.⁵⁹ In this same half-decade, Robert Fulton ordered a marine engine from Boulton and Watt, and by 1807 he had introduced his more successful steamboat.

As an outgrowth of these initiatives, commercialization of engine building began as early as 1808 in the United States. The opening of Evans's Mars Works in Philadelphia gave American manufacturers access to high-pressure, relatively safe, and relatively fuel-efficient steam technology. When Evans opened a Pittsburgh shop in 1809, midwestern manufacturers also had access to engines and parts without the surcharge of long-distance transportation. There is evidence that engine technology had penetrated as far as the markets of the Pacific coast by the early 1840s so that nowhere in the nation was it considered either "novel" or "experimental."⁶⁰ Nevertheless, indigenous production did not promote or ensure extensive, uniform, or quick adoption in manufacturing in either England or the United States—or, for that matter, in Western Europe, where between 1810 and 1825 full-scale rotary engine industries had become established in Belgium, France, Germany, and Russia. The German government even subsidized engine production in state factories to fill home demand.⁶¹ Still, steam received a mixed reception in manufacturing.

Delays in diversified industrial adoption in these technologically more sophisticated nations resulted not from inadequate information or engineering skills but from the low level of demand for steam at existing costs and in terms of existing needs. Certainly a lack of demand in the United States was consonant with capital scarcities and with the increased productivity of labor using traditional power technology. The mix of capital constraints, high productivity, and the availability of satisfactory alternative power sources provides a much more likely explanation for delayed adoptions of steam than supply-side evidences of diffusion difficulties, difficulties that do not adequately account for the relatively low number of steam engines in use prior to 1850. In 1838 there were at most 1,420 stationary steam engines in the United States, and by 1850 the figure still totaled only 8,598; in 1880, however, there were 56,123 engines of greater capacity in industrial use.⁶²

There is little doubt that advanced steam engineering had its most immediate and far-reaching effect—on both sides of the Atlantic—in transportation, for it enabled the opening and integration of distant domestic markets.⁶³ By the 1820s, steamboats in England and in the United States were carrying goods to scattered

⁵⁹ For Evans's classic on American flour mills, see his *The Young Mill-Wright and Miller's Guide* (1850; reprint edn., New York, 1972). For his career and concern with steam technology, see Pursell, *Early Stationary Steam Engines*, 44–47; and Greville Bathe and Dorothy Bathe, *Oliver Evans: A Chronicle of Early Engineering* (Philadelphia, 1935; reprint edn., New York, 1972), 65–128.

⁶⁰ Pursell, *Early Stationary Steam Engines*, 62–68; and Atack *et al.*, "Regional Diffusion and Adoption of the Steam Engine," 286–88.

⁶¹ Tann and Breckin, "The International Diffusion of the Watt Engine," 556–60.

⁶² Atack *et al.*, "Regional Diffusion and Adoption of the Steam Engine," 283–85.

⁶³ Louis Hunter, *Steamboats on the Western Rivers* (Cambridge, Mass., 1949); Erik F. Haites and James Mak, "The Decline of Steamboating on the Antebellum Western Rivers: Some New Evidence and an Alternative Hypothesis," *EEH*, 11 (1973–74): 25–36; and Rostow, *The Stages of Economic Growth*. Also see Robert Fogel, *Railroads and American Economic Growth: Essays in Econometric History* (Baltimore, 1964); and Paul A. David, "The Growth of Real Product in the United States before 1840: New Evidence, Controlled Conjectures," *JEH*, 27 (1967): 151–97.

urban centers for further distribution, and by mid-century railroads had added to the importance of steam in developing commercial networks. Less certain and still to be established are the various backward and forward linkages of steam to industrial growth. What we can point to, however, is the overlooked, if prolonged interval (from roughly the 1780s to the 1870s), for steam to go from 1 to 50 percent of the industrial motive power market both in England and in the United States.

Not only was there a very long lead time of about one hundred years in the structural evolution of this new power system, but in both nations the diffusion of steam technology was also chronologically congruent. These similar patterns give historical support to logistic analysis that posits a stable time constant of about a century for new energy sources to penetrate established arrangements.⁶⁴ Granting transatlantic variations and specific regional differences, we can construct virtually identical trends for both countries. For example, as late as the 1830s manufacturers in each nation's leading industries, such as textiles, iron smelting, and flour milling, much preferred waterwheels to engines, except for the notable preference for steam in English cotton spinning. Then, beginning in the 1840s, steam began to take hold where water was unavailable or noncompetitive, as in the American Midwest, or where the power-intensive needs of particular industries demanded supplementary supplies. Large-scale operators in Lancashire and Lowell began meeting increasing power needs with steam, but in neither instance did they literally "switch," so that steam added to but did not replace waterpower. The monetary costs of steam started declining in both countries, but, in both, alternative power costs were sufficiently close so that other considerations, such as custom and terrain, figured prominently in power choices. Cotton mills near Lowell, for example, with leases of long-term water rights, and dams in place, took advantage of good "heads" as a trade-off for the cost of transporting raw cotton to the mill and shipping out manufactured goods.⁶⁵

Industrial adoption of the steam engine slowly increased in the 1840s, yet steam did not surpass water—or, probably, muscle—power by mid-century. Large-scale substitution for traditional, renewable sources of power became significant after, not before, the watershed of 1850, when both the U.S. and British economies had already reached maturity. In fact, in 1850, a tiny fraction, some 181,000 steam horsepower, was probably generated in all American factories to drive machines at a time when inanimate sources supplied 2.5 million horsepower and the total horsepower of all sources, including humanpower, reached 8.5 million. The turning point in the United States was first reached in 1870, when steam horsepower of 1,215,711 in manufacturing barely edged out water's use of 1,130,431 horsepower.⁶⁶

⁶⁴ Marchetti and Nakicenovic, *The Dynamics of Energy Systems*, 7, 15.

⁶⁵ Von Tunzelmann, *Steam Power and British Industrialization to 1860*, 141, 171–73, 176–78, 201, 282–92; and Atack *et al.*, "Regional Diffusion and Adoption of the Steam Engine," 288–300.

⁶⁶ Atack *et al.*, "Regional Diffusion and Adoption of the Steam Engine," 283, 297; and Dewhurst *et al.*, *America's Needs and Resources*, 1117. By 1918 the potential of waterpower for generating electricity was advocated in a call for an integrated economic and energy policy. It is interesting to find how precisely the authors' evolutionary assumptions and references to slave labor echo similar nineteenth-century analogies: "To accomplish the work done annually in the United States, or at least the equivalent in such kind as men could

The great, gleaming Corliss engine dominating Machinery Hall at the Philadelphia Centennial Exhibition in 1876 epitomized for everyone the dominance of steam. Its capacity of 2,500 horsepower and the machine tools beside it suggested a range of possibilities still to come. Although the trend to steam seemed confirmed, 1880 was the first year in which this new source contributed 60 percent of the motive power in manufacturing, and, by the time of its complete diffusion in 1890, electricity had already become a potent competitive threat.⁶⁷

The British data are not, of course, directly comparable, and the magnitudes differ. According to Musson's revised estimates, in 1850 "nominal" industrial steam horsepower in England came to 300,000. Of this total, 108,000 horsepower was located in textiles, while lesser amounts were distributed among coal, iron, and engineering.⁶⁸ The figures seem surprisingly small, as does an even higher estimate of 500,000 "indicated" steam horsepower, next to suppositions of "massive growth" in the "Age of Steam" or to estimates of 750,000 horsepower used by locomotives by mid-century. As in the United States, the first half-way mark for steam as a share of industrial motive power came as late as 1870. By then, motive power capacity in England, excluding animate sources, reached an estimated 2.2 million; about 1.2 million was used in manufacturing, and steam probably accounted for half of that.⁶⁹

Even where the locational advantages of steam seem clear, as in urban manufacturing, engine adoption was slow; analogous Anglo-American patterns of constraint appear in both the pace and the extent of urban usage. Whatever our preconceptions about the great shift of factories from rural hillsides to the city during the Industrial Revolution, they do not correlate with industrial adoption of steam. The move to steam-powered mechanization came slowly, and even in the largest metropolitan centers the mobility of steam engines, in contrast to technologies fixed by terrain (such as water and wind), did not result in either early or extensive application. What is thought of as "the almost universal use of steam engines for power" that concentrated factory manufacturing in sooty cities, or what Engels called "the Great Towns,"⁷⁰ came belatedly.

Prior to 1810, urban industrial utilization of steam existed only in isolated instances—a steam-powered sawmill to cut lumber in New York, a small establishment in Philadelphia that employed steam for grinding plaster of Paris, and a few foundries in London. There were also a few small-scale, multipurpose establishments, such as the Mars Works in Philadelphia, which, in addition to building engines, became an iron foundry, a blacksmith, and a mill-stone manufacturer. In

perform, would require the labor of three billion hard-working slaves. The use of power gives to each man, woman, and child in this country the service equivalent of thirty servants. Modern civilization arises from this organized employment of mechanical energy." Chester G. Gilbert and Joseph E. Pogue, *Power: Its Significance and Needs*, United States National Museum, Bulletin no. 102, pt. 5 (Washington, 1918), 1–8.

⁶⁷ Robert C. Post, ed., *1876: A Centennial Exhibition* (Washington, 1976), 223; and Robert Thurston, *A History of the Growth of the Steam Engine* (Ithaca, N.Y., 1939), 502–25.

⁶⁸ Musson, "Industrial Motive Power in the United Kingdom," 435.

⁶⁹ Kanefsky, "Motive Power in British Industry," 373–75.

⁷⁰ Wallace, *Rockdale: The Growth of an American Village*, 5; Kasson, *Civilizing the Machine*, 69; and Friedrich Engels, *The Condition of the Working Class in England*, trans. and ed. W. O. Henderson and W. H. Chaloner (Stanford, 1958), chap. 3.

London, similarly, there were a few industrial shops, but steam served mainly for pumping operations in breweries, distilleries, waterworks, and mills.⁷¹

The low level of urban industrial use stands out into the 1820s and 1830s, even as steampower gradually extended into a wider spectrum of activity. In these middle years, the cost of bringing in labor-intensive wood or coal fuel was but one handicap that retarded the spatial concentration of engines in cities. The structure of industry and methods of production figured just as prominently. Manufacturing in London, New York, and other commercial centers depended on skilled craftsmen who had little need for large-scale power supplies. In Boston as late as 1845, the majority of industrial laborers worked at home or in small neighborhood shops; the same was true in Philadelphia, Baltimore, and London. There were, of course, exceptions. In New York, steam became important for driving printing presses and in running saws for lumber milling. There were even cases of steam-driven cotton mills in Baltimore, Newport, and Philadelphia. But the most important metropolitan industries—such as boots, shoes, and clothing—were completely unmechanized in both Britain and the United States at mid-century.

Little can be claimed for the locational mobility of steam technology in the growth of urban manufacturing in the United States. By 1832 only four of Boston's ninety-five industrial categories employed steam, and by 1838 all forty-six of the city's steam-powered establishments generated a total of 429 horsepower. The profile for Baltimore, Philadelphia, and New York—the other largest cities in the United States—is much the same.⁷² Only where falling water for power was scarce, as in the Pittsburgh area, did steam stimulate metropolitan development earlier than 1850. Even given the regional variations, however, as late as 1860 fewer than twelve cities in the United States with populations of 10,000 or more could point to any substantial industrial sector; in the largest cities, such as Philadelphia and New York, only one-fifth to one-eighth of the population was engaged in manufactures.⁷³

Stationary steam engines had considerably less impact on urbanization than did locomotives and steamboats. In fact, big cities in both England and the United States grew preeminently as mercantile centers during the Industrial Revolution. And, although urban handicraft manufacturing remained limited and subordinate to trade and finance in these cities, wherever rural, mechanized, water-powered industry was within a reasonable distance of good transportation facilities, its output boosted urban commercial activity. Prospering entrepôts for overseas and domestic trade, such as London, New York, and Boston, all benefited from traditionally powered, well-located rural industry. Of course, English cities used more steampower earlier and extended it into a wider array of activities, but we still do not know the distribution between manufacturing and water-pumping tasks. In most cities, moreover, manual labor predominated, and even Birmingham, the hub

⁷¹ Allan R. Pred, *The Spatial Dynamics of United States Urban Industrial Growth, 1800–1914* (Cambridge, Mass., 1966), 158–61, and *Urban Growth and the Circulation of Information: The United States System of Cities, 1790–1840* (Cambridge, Mass., 1973), 239–83; and Musson, "Industrial Motive Power in the United Kingdom," 425–28.

⁷² Pred, *Spatial Dynamics of United States Urban Industrial Growth*, 161.

⁷³ Hunter, *A History of Industrial Power*, 485–87.

of metal working and the site of Boulton and Watt's Soho factory, did not become a center of steam-powered industry in the first half of the century. Instead of becoming concentrated industrial centers, major urban areas continued to mirror their commercial and confined manufacturing base.⁷⁴

Steam did not attract industry to urban locations until problems of cost, capital, and technical inefficiencies could be overcome to meet the demands of a changing marketplace. The 1870s first saw the concentration of large-scale, steam-powered manufacturing in cities, and, by then, the beginnings of spatial density paralleled a myriad of other changes, including the move to big business, the initiation of transatlantic telegraphic communication, and the completion of national transportation systems feeding into widening international markets.

RELIANCE ON TRADITIONAL POWER for much of the nineteenth century is one of the most outstanding, if one of the most neglected, characteristics of the industrializing process in Britain and in the United States. What has long been believed to be a large-scale shift from handicraft to machine techniques, and from animate to inanimate sources of power, occurred irregularly or, most often, not at all in major areas of Anglo-American manufacturing. Clearly, the new did not rapidly become a substitute for, or substantially displace, the old in either of the first two nations in the Western world to industrialize. Instead, in these expanding transatlantic economies the growing need for motive power ensured the cumulative, yet always competitive exploitation of diverse energy sources.

Increased output, rather than increases in steam utilization, is the benchmark of the Industrial Revolution until 1850. And, as Marx was one of the first to recognize, power, regardless of its source, figured merely as the means to this end. In spite of the traditional triad of the Age of Steam, which links power, productivity, and progress, steam's industrial use can hardly be credited with setting in motion the rapid, widespread transformation that we refer to as the Industrial Revolution. Indeed, these new perspectives on Anglo-American power patterns should bring into question hypotheses that take for granted either new energy systems or extensive application of fossil fuels as prerequisites for progress.

⁷⁴ Von Tunzelmann, *Steam Power and British Industrialization to 1860*, 31–36; and Musson, "Industrial Motive Power in the United Kingdom," 425–29.

The Puzzle of the American Climate in the Early Colonial Period

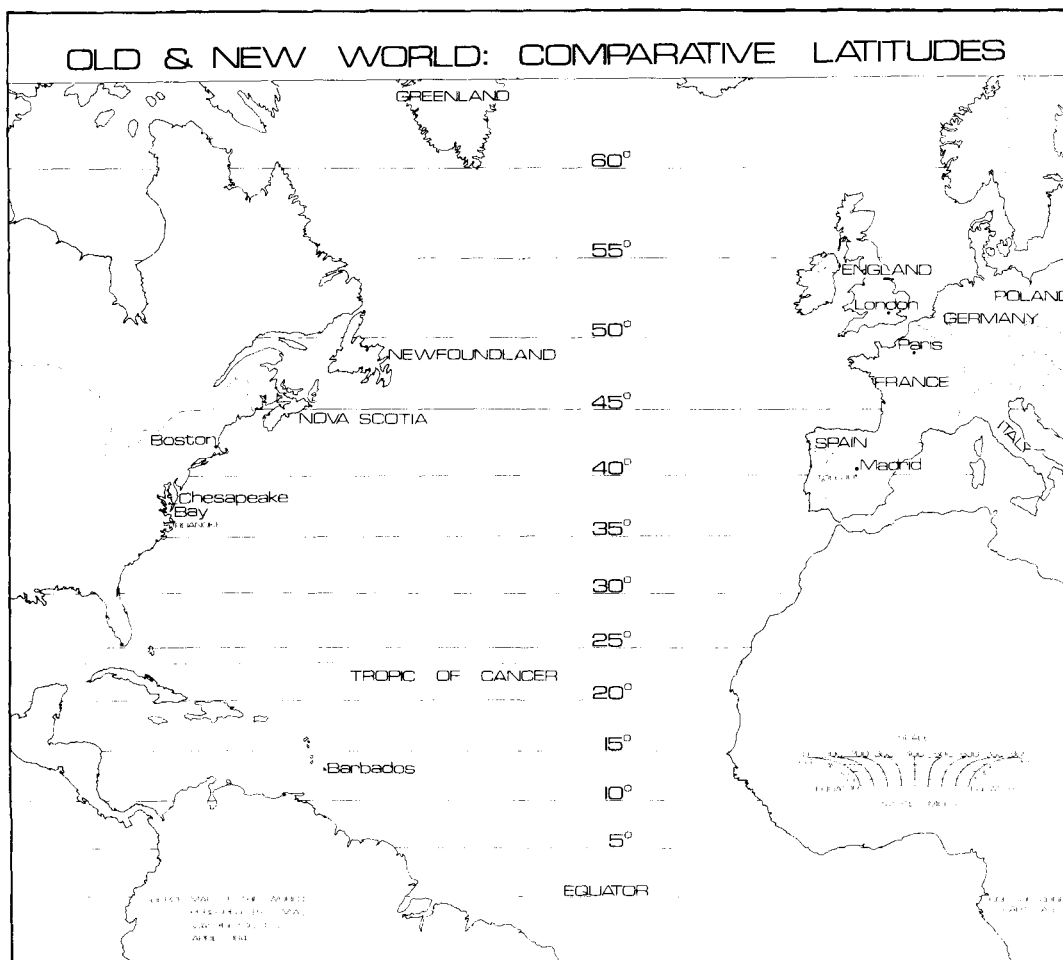
KAREN ORDAHL KUPPERMAN

ENGLISH EXPECTATIONS ABOUT WEATHER PATTERNS in North America were based on the common-sense assumption that climate is constant in any latitude around the globe. Newfoundland, which is south of London, was expected to have a moderate climate, and Virginia was expected to be like southern Spain. Under stress produced by high death rates, the failure of settlements, and pressure from investors disappointed by the colonies' inability to produce the rich commodities associated in their minds with hot climates, colonists eventually had to acknowledge that, although it is as hot in summer as the English anticipated, the eastern mainland of North America is much colder in winter, spring, and fall. In order to understand their situation, they began very early to compile information not only about weather patterns but also about the growing season and the types of crops that they could produce. And yet they clung with persistence to inherited notions about climate that their own experience contradicted. The process by which ideas about climate, some of which had roots in classical literature, were modified or discarded under the impact of experience is a fascinating chapter in the history of scientific thought. For the colonists themselves, it was the story of a mental adjustment that was both slow and costly in money as well as lives.

In order to assess the adjustment required of early colonists, it is important to understand the characteristic climates of Western Europe and eastern North America and the factors that determine them, including what historians of climate have determined about conditions in both regions during what has been called the "little ice age" of the sixteenth and seventeenth centuries.

EASTERN NORTH AMERICA, with its extremes of heat and cold, has a continental climate, whereas England and all other European countries bordering the Atlantic

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have an oceanic climate. The difference is principally due to the general west to east circulation of the atmosphere in these latitudes. Eastern America's weather is governed by air masses approaching over land, that of Europe's west coast by air masses originating over water. The atmosphere is heated indirectly, by transfer from the land or sea below. Water absorbs and releases heat much more slowly than land; therefore, the overlying air is warmer in winter and cooler in summer. The differential absorption and radiation properties of land and sea are so great as to produce two quite different climates. The oceanic climate of Western Europe, marked by small daily and seasonal ranges of temperature and a lag in the onset of the seasons, is humid, with adequate rainfall in all seasons. Ocean currents, such as the Gulf Stream, may also influence oceanic climates.¹

By contrast, the temperate continental climate of eastern North America is prone to extremes—a “genuine winter” with snow mantle and an “authentic summer,” interspersed with a short spring and fall, and with wide variations from year to

¹ For a discussion of the role of the Gulf Stream, see Henry Chapin and F. G. Walton Smith, *The Ocean River: The Story of the Gulf Stream* (New York, 1952); and Henry Stommel, *The Gulf Stream: A Physical and Dynamical Description* (2d edn., Berkeley and Los Angeles, 1965), 173–76.

year. The temperate continental climate occurs between 35 and 50 degrees of latitude in North America, whereas the comparable zone is further north, 40 to 60 degrees, in Western Europe. It is subhumid, which could mean too little precipitation, although this danger should be least on the east coast. Rather than being spread out through all four seasons, precipitation tends to be greatest during the summer, when there is maximum danger of losing water through evaporation. Therefore, precipitation may be much less effective in such a climate.²

The late sixteenth and early seventeenth centuries were the time of what has been called the "little ice age" in the northern hemisphere, making it an inauspicious period for the founding of colonies. The little ice age is given various beginning and ending dates by historians of climate, most making it as broad as from the middle of the fifteenth to the middle of the nineteenth century.³ Because there was no unbroken run of cold winters and cool, wet summers, experts disagree about the outer limits of the period, but all agree that its greatest intensity was felt between 1550 and 1700.⁴ Severest winter cold and least summer warmth occurred in the 1590s, the first decade of the seventeenth century, the 1640s, and the last decade of the seventeenth century. The global mean temperature for the period was probably about one degree centigrade lower than in the relatively warm period from 1890 to 1950.⁵

Emmanuel Le Roy Ladurie has suggested that the designation of this period as the little ice age gives an impression of greater severity than is warranted and that a drop of one degree in mean world temperatures would not be sufficient to cause real distress. He has simply noted the years 1550–1700 as a period of glacial advance and somewhat cooler temperatures.⁶ Other historians have countered that a drop of even one degree centigrade can result in an appreciably shorter growing season. During the period called the little ice age, cereal growing died out in Iceland, where it had been established, and within Britain and Scandinavia the amount of land under cultivation diminished. H. H. Lamb has estimated that the growing season was probably three to four weeks shorter in the English lowlands during the coldest part of the little ice age than it was in the period 1930–49 and that it was shortened even more drastically in the uplands of northern England and Scotland. We know that there were incidents of famine in northern England and

² This discussion is based on Glenn T. Trewartha, *An Introduction to Climate* (4th edn., New York, 1968), 18–38, 306–38; H. H. Lamb, *The Changing Climate* (London, 1966), 45–46, and *Climate: Present, Past, and Future*, 2 vols. (London, 1972–77), 1: 83, 136, 328; and Carl O. Sauer, "The Settlement of the Humid East," in *Climate and Man*, Yearbook of Agriculture, 1941 (Washington, 1941), 159. Discussion with John L. Allen greatly clarified the issues for me.

³ Nigel Calder, *The Weather Machine* (London, 1974), 16–17; John Gribbin and H. H. Lamb, "Climatic Change in Historical Times," in Gribbin, ed., *Climatic Change* (Cambridge, 1978), 70–71; and John Imbrie and Katherine Palmer Imbrie, *Ice Ages: Solving the Mystery* (Short Hills, N.J., 1979), 181.

⁴ Lamb, *The Changing Climate*, 65, 144, and *Climate: Present, Past, and Future*, 2: 463; John E. Oliver, *Climate and Man's Environment: An Introduction to Applied Climatology* (New York, 1973), 366; Emmanuel Le Roy Ladurie, *Times of Feast, Times of Famine: A History of Climate since the Year 1000*, trans. Barbara Bray (New York, 1971), 233–37; M. L. Parry, *Climatic Change, Agriculture, and Settlement* (Hamden, Conn., 1978), 38, 66; and Andrew B. Appleby, "Epidemics and Famine in the Little Ice Age," *Journal of Interdisciplinary History*, 10 (1980): 645.

⁵ Calder, *The Weather Machine*, 16–17; Parry, *Climatic Change, Agriculture, and Settlement*, 39; and Le Roy Ladurie, *Times of Feast, Times of Famine*, 90, 165, 173, 227, 236, 243.

⁶ Le Roy Ladurie, *Times of Feast, Times of Famine*, 220–24, 288–89; and Appleby, "Epidemics and Famine in the Little Ice Age," 645n.

Scandinavia.⁷ Certainly, seventeenth-century Europeans were aware of the cold trend of their weather. The Thames, which had never frozen more than once or twice in a century, froze solid four times in the sixteenth, eight times in the seventeenth, and six times in the eighteenth century. During the seventeenth century, people held “frost fairs” on the frozen Thames. Pollen evidence shows that North America experienced a period of cold that coincided with the little ice age in Europe,⁸ and English colonists reported many instances of extreme weather.

Historians of climate now agree that the idea of “normal” climate is an illusion: the climate over large sections of the globe fluctuates, with alternating warmer and cooler periods. The little ice age followed the medieval optimum, a warm period in the northern hemisphere during which Scandinavian influence spread over the North Atlantic and Europe and American Indians cultivated the Great Plains intensively. The interval from about 1890 to about 1950 was another warm period, from which the northern hemisphere may again be moving into colder times. These variations may be caused by small changes in the sun’s energy output, whereas major ice ages are the product of changes in the earth’s orbit and attitude to the sun.⁹

ENGLISH WRITERS INITIALLY USED THE WORD “climate” almost interchangeably with “latitude.” In fact, *climata* were the parallel bands into which classical authors divided the inhabited world. Although climate came to be associated with the temperature of the air, which usually varies in a predictable way with latitude, *climata* were “artificial astronomical divisions the boundaries of which were determined by arbitrary means.” Almanacs published in the seventeenth century often showed a globe divided into seven climates.¹⁰ Even though medieval writers such as Giralduus Cambrensis and Roger Bacon understood that topography influences climate and that west sides of continents differ from east sides, English writers approaching America confidently believed they could extrapolate from the familiar climates of Europe to predict the climate of North America.¹¹

⁷ Lamb, *Climate: Present, Past, and Future*, 2: 477n.; Parry, *Climatic Change, Agriculture, and Settlement*, 20–23, 108, 119–25, 128–34; Reid A. Bryson and Christine Padoch, “On the Climates of History,” *Journal of Interdisciplinary History*, 10 (1980): 589; Andrew B. Appleby, *Famine in Tudor and Stuart England* (Stanford, 1978); and Gustaf Uterström, “Climatic Fluctuations and Population Problems in Early Modern History,” *Scandinavian Economic History Review*, 3 (1955): 3–47.

⁸ Lamb, *The Changing Climate*, 11; Parry, *Climatic Change, Agriculture, and Settlement*, 163; Crispin Tickell, *Climatic Change and World Affairs*, Harvard Studies in International Affairs, no. 37 (Cambridge, Mass., 1977), 14; and Thompson Webb III, “The Reconstruction of Climatic Sequences from Botanical Data,” *Journal of Interdisciplinary History*, 10 (1980): 765–66. Mean world temperatures during the Great Ice Age at the maximum period of glaciation were six degrees centigrade lower than twentieth-century temperatures.

⁹ John Gribbin, *What’s Wrong with Our Weather? The Climatic Threat of the 21st Century* (New York, 1979), 36–42; Bryson and Padoch, “On the Climates of History,” 596–97; Parry, *Climatic Change, Agriculture, and Settlement*, 147–54; and Imbrie and Imbrie, *Ice Ages*, 161–73, 183–84.

¹⁰ John K. Wright, *The Geographical Lore of the Time of the Crusades* (New York, 1925), 23; Clarence Glacken, *Traces on the Rhodian Shore* (Berkeley and Los Angeles, 1967), xv; H. Howard Frisinger, *The History of Meteorology to 1800* (New York, 1977), 28; and Bernard Capp, *English Almanacs, 1500–1800: Astrology and the Popular Press* (Ithaca, N.Y., 1979), 203.

¹¹ Philip Vincent, *A True Relation of the Late Battel Fought in New England, between the English, and the Salvages* (London, 1637), sig. A4; Edward Hayes and Christopher Carleill, “A Discourse concerning a Voyage Intended for the Planting of Chrystian Religion and People in the North West Regions of America in Places Most Apt for

Climate was of immense importance to the English who wanted to colonize America, because they believed their self-definition was at stake. English culture was seen as the product of England's temperate climate, which fostered "mechanicall and politike Arts" and the further humanizing of society.¹² It was commonly believed that the English could thrive only in moderate circumstances, at the mean of heat and cold, high land and low. If they were to move into an area of tropical heat and humidity, their essential character would disintegrate. The French and Spaniards were suited to such areas, both culturally and physically, but the English were not. At the worst, English people might die, even at 40 degrees of latitude. At the least, they could become like the despised Spaniard. Edward Hayes believed that England should not hope for gold and silver from North America because such metals are usually found in "hoatt and untemperate Regions." They should value "health of boddy and delyght" more than their greed for gold, because if they were to move in the "burning Zoanes" the heat and air would be "unto our Complexions intemperat and Contagious. Nature hath framed the Spaniards apt to suche places. Who prosper in drye and burning habitations. But in us she abhorreth suche." Thomas Morton put the contrast another way: "This *Torrída Zona* is good for Grashoppers: and *Zona Temperata* for the Ant and Bee."¹³

The connection between climate and character was supported by medical lore, which held that good health is produced by the balance of the four humors in the human body. Illness occurs when some external force, often a change of climate or air, unbalances them. Since each climate has its characteristic balance between the humors, permanent changes of climate produce permanent changes of character; an Englishman moving to a hot climate would become more fiery and quick-tempered as well as indolent. Sir Henry Colt was appalled by the "land of discord," Barbados: "Who is he that cann live long in quiet in these parts? For all men are heer made subject to ye power of this Infernall Spiritt. And fight they must, although it be with ther owne frends."¹⁴

Fears that the American climate would adversely affect English character were balanced by plans for commodities that warmer colonies might produce. Since colonial promoters assumed climate would be the same in any given latitude

the Constitution of Our Boddies, and the Spedy Advancement of a State," in D. B. Quinn *et al.*, eds., *New American World: A Documentary History of North America to 1612*, 5 vols. (New York, 1979), 3: 158–60; Wright, *Geographical Lore of the Time of the Crusades*, 177; and Frisinger, *History of Meteorology to 1800*, 33.

¹² Samuel Purchas, *Hakluytus Posthumous: or, Purchas His Pilgrimes*, 20 vols. (1625; Glasgow, 1905–06), 1: 162.

¹³ Hayes and Christopher Carleill, *A Treatise Conteyning Important Inducements for the Planting in These Parts* (1602), in John Brereton, *A Briefe and True Relation of the Discoverie of the North Part of Virginia* (2d edn., London, 1602), 15, and "Discourse concerning a Voyage," 159; Morton, *New English Canaan* (1637), in Peter Force, comp., *Tracts and Other Papers, Relating Principally to the Origin, Settlement, and Progress of the Colonies in North America*, 4 vols. (1836; reprint edn., Gloucester, Mass., 1963), 2: 13; and Sir Ferdinando Gorges, *A Briefe Relation of the Discovery and Plantation of New England* (1622), in James Phinney Baxter, ed., *Sir Ferdinando Gorges and His Province of Maine*, 3 vols. (1890; reprint edn., New York, 1967), 1: 228. The pale faces of Virginia colonists who came to Massachusetts on trading voyages were evidence for William Wood that their constitutions were already changing; Wood, *New Englands Prospect* (London, 1634), 8–9.

¹⁴ Colt, "The Voyage of Sr. Henry Colt Knight to ye Ilands of ye Antilles in ye Shipp called ye Alexander, wherof William Burch Was Captayne & Robert Shapton Master. Accompanied with Divers Captaynes & Gentlemen of Note" (1631), in V. T. Harlow, ed., *Colonising Expeditions to the West Indies and Guiana, 1623–1667* (London, 1923), 73, 76; Sir William Vaughan, *The Newlanders Cure* (London, 1630), 6; Thomas West, Baron de

around the world, they also assumed that the products—animal, vegetable, and mineral—would be the same. Hopes centered on Virginia and other southern areas. Many writers spoke of gold, silver, and pearls, justifying their expectations by reference to Persia, Japan, and China. The warm sun “doth norishe and bryng fourth gold, spices, stones and perles,” or, as the Virginia Company put it, the sun is “under god the first cause both of health and Riches.”¹⁵ Colonial promoters expected colonists to produce wine, silk, olive oil, sugar, and spices—products that England then bought from Spain, Portugal, and France. Having colonies capable of providing these commodities would free England from its reliance on potential enemies. The two Richard Hakluyts hammered on this point constantly, always drawing on their equation of climate and latitude.¹⁶ Finding a reliable source of oil was particularly crucial to the English economy. Large quantities of soap were used in the production of woolen cloth, and a sweet-smelling oil was needed for its manufacture. Fish oil was unsuitable, because the strong smell could not be eliminated from the finished wool. Thus, an important aspect of England’s economy was dependent on the “vile Portingall” and the “perfidious Spaniard.”¹⁷

Early experience did not destroy the belief that tropical and Mediterranean crops would grow in Virginia. Thomas Hariot and Ralph Lane maintained that such plants were not found there because the Indians did not have the knowledge to develop the area’s potential. Both also said that important finds were being kept secret for the time being. Early descriptions of Jamestown assumed that the colony could grow crops from other countries in the same latitude.¹⁸ Reports from the West Indies confirmed that the desired products would grow there, but these areas were seen as dangerous because of their unaccustomed heat and their proximity to

la Warr, *The Relation of the Right Honourable the Lord De-La-Warre* (1611), in Alexander Brown, ed., *The Genesis of the United States*, 2 vols. (1890; reprint edn., New York, 1964), 1: 480; Vincent, *True Relation of the Late Battell*, sig. bv; Glacken, *Traces on the Rhodian Shore*, 12, 449–50; Wright, *Geographical Love of the Time of the Crusades*, 180; and Samuel Purchas, *Purchas His Pilgrimage* (2d edn., London, 1614), 760. For a powerful description of the actual adjustment of English colonists to the semi-tropical climate of the West Indies, see Richard S. Dunn, *Sugar and Slaves: The Rise of the Planter Class in the English West Indies, 1624–1713* (Chapel Hill, 1972), esp. chaps. 8–9.

¹⁵ Susan Myra Kingsbury, ed., *Records of the Virginia Company*, 4 vols. (Washington, 1906–35), 3: 15; and Roger Barlow, *A Briefe Summe of Geographie* (1541), ed. E. G. R. Taylor (London, 1932), 179–80.

¹⁶ Richard Hakluyt, the elder, *Notes Framed by a Gentleman* (1578), in D. B. Quinn, ed., *The Voyages and Colonising Enterprises of Sir Humphrey Gilbert*, 2 vols. (London, 1940), 1: 184, and *Inducements to the Liking of the Voyage Intended towards Virginia* (1585), in E. G. R. Taylor, ed., *The Original Writings and Correspondence of the Two Richard Hakluyts*, 2 vols. (London, 1935), 2: 335; and Richard Hakluyt, *A Particular Discourse concerninge the Western Discoveries*, known as the *Discourse of Western Planting* (1584), *ibid.*, 313, and “Epistle Dedicatory” to Sir Robert Cecil, *Principal Navigations* (1599), *ibid.*, 456. Also see Robert Detweiler, “Was Richard Hakluyt a Negative Influence in the Colonization of Virginia?” *North Carolina Historical Review*, 158 (1971): 359–69.

¹⁷ David B. Quinn, *England and the Discovery of America, 1481–1620* (London, 1974), 320–21; Christopher Carleill, *A Briefe and Summary Discourse upon the Intended Voyage to the Hithermost Parts of America*, in Quinn, *Colonising Enterprises of Sir Humphrey Gilbert*, 2: 353–54; Hayes and Carleill, “Discourse concerning a Voyage,” 164–65, 171; and Virginia Company, *A True Declaration of the Estate of the Colonie of Virginia* (1610), in Force, *Tracts and Other Papers*, 3: 23. For an extended discussion of this point, see Karen Ordahl Kupperman, “English Perceptions of Treachery, 1583–1640: The Case of the American ‘Savages,’” *Historical Journal*, 20 (1977): 278–82.

¹⁸ Hariot, *A Briefe and True Report of the New Found Land of Virginia* (1588), in David Beers Quinn, ed., *The Roanoke Voyages, 1584–1590*, 2 vols. (London, 1955), 1: 325–26, 336–37, 383; Lane, “An Extract of Master Lanes Letter from Virginia” (1585), *ibid.*, 208–09; Virginia Company, *True Declaration of Estate*, 22–23; and Gabriel Archer, *The Description of the Now Discovered River and Country of Virginia* (1607), in Philip L. Barbour, ed., *The Jamestown Voyages under the First Charter, 1606–1609*, 2 vols. (Cambridge, 1969), 1: 101.

Spanish bases. The very heat that created abundance also made life difficult for transplanted English settlers.¹⁹ Virginia territory was seen as safer; it offered wealth-giving heat balanced by a cold winter season. From the beginning, colonists acted on their beliefs about what would grow in the South. Ships headed for Roanoke in the mid-1580s called at the West Indies, where they picked up various tropical plants for testing in North Carolina. Failure of these tests was not seen as serious. Hariot explained that the sugar cane deteriorated in transit and the proper time for planting had passed. The latitude of Roanoke convinced him that oranges and lemons as well as sugar cane would surely grow well and that there would soon be “no small commodities in Sugers Suckers, and Marmalades.”²⁰

Richard Hakluyt pointed out that the Spanish and Portuguese had introduced sugar, ginger, woad, cattle, and grape vines into the Azores, Hispaniola, Madeira, Brazil, St. Thomas, and “sondry other places,” where none had grown when these regions were first discovered. Since the Spanish and Portuguese now had a thriving trade in such commodities, the English on the mainland of North America assumed that they, with care and attention, could do the same.²¹ Colonists were also encouraged to investigate and upgrade products native to Virginia. Thomas Hariot, who was at Roanoke in 1585–86, reported a kind of silk grass similar to one grown in Persia, “which is in the selfe same climate as Virginia.” He believed it would prove marketable and would be a source of “great profite.” He was also interested in developing native flax. From the founding of Jamestown in 1607 to the revocation of its charter in 1625, the Virginia Company tried to develop the silk grass, ordering all householders to plant it and importing skilled men from the Low Countries to work with it.²²

The Jamestown colonists continued their efforts to transplant West Indian commodities. Gabriel Archer and William Strachey reported early success, but, although the Virginia Company as late as 1620 was still planning to import olive trees into Virginia, the tropical plants experiment was mostly a failure because the winters were too severe. As Gary S. Dunbar has pointed out, the few successes were accidental; they were plants that, despite their tropical origins, were tolerant of a wide range of conditions.²³ Quite early, the Virginia Company concentrated its

¹⁹ Colt, “Voyage . . . to ye Ilands of ye Antilleas,” 67; [Major John Scott] “The Discription of Trinidad,” in Harlow, *Colonising Expeditions to the West Indies*, 120, 125, and “The Discription of Guyana,” *ibid.*, 134–35; Captain William Jackson, *A Briefe Journall of the Voyage Undertaken by Captaine William Jackson to the Westerne Indies or Continent of America* (1642), ed. V. T. Harlow, *Camden Miscellany*, 13 (1923): 18; and Sylvester Jourdain, *A Discovery of the Bermudas, Otherwise Called the Isle of Devils* (1610), in Louis B. Wright, ed., *A Voyage to Virginia in 1609* (Charlottesville, Va., 1964), 110–13. I am working on a separate article on heat and attitudes toward it in the same period.

²⁰ Hariot, *Briefe and True Report*, 336; John White, *The Fourth Voyage Made to Virginia* (1587), in Quinn, *The Roanoke Voyages*, 2: 520–21; and *The Voyage Made by Sir Richard Greenville* (1585), *ibid.*, 187, 187n.

²¹ Hakluyt, “Epistle Dedicatory” to R. Laudonnière, *A Notable Historie containing Foure Voyages unto Florida*, in Taylor, *Writings and Correspondence of the Two Richard Hakluyts*, 2: 374; and Ligon, *A True and Exact History of the Island of Barbados* (London, 1657), 84–86.

²² Hariot, *Briefe and True Report*, 325–27; Kingsbury, *Records of the Virginia Company*, 3: 166, 254, and 4: 15; Ralph Hamor, *A True Discourse of the Present Estate of Virginia* (London, 1615), 35; and Robert Johnson, *The New Life of Virginea* (1612), in Force, *Tracts and Other Papers*, 1: 14. For discussions of the silk grass, see Quinn, *The Roanoke Voyages*, 1: 325 n. 4, and *England and the Discovery of America*, 463; and Gary S. Dunbar, *Historical Geography of the North Carolina Outer Banks* (Baton Rouge, 1958), 117–18 n. 66.

²³ Archer, *Discription of the Now Discovered River and Country of Virginia*, 100; Strachey, *The History of Travell into Virginia Britania*, ed. Louis B. Wright and Virginia Freund (London, 1953), 38–39; Robert Johnson, *Nova*

attention on wine grape and silk production. These had more reasonable chances of success and, in fact, experiments with grape vines and mulberry trees were being conducted in England at the same time.²⁴ Colonists were encouraged by the native vines and mulberry trees they found in Virginia. Surely, they reasoned, their presence demonstrated that the land could produce wine and silk. The king and the company issued many orders to plant and care for imported varieties of both plants and made it clear that no excuse for failure would be accepted. Despite many reports that the transplants were thriving, the business did not prosper. The mulberry trees grew, but the silk worms died. Most of the blame must be attributed to the greater success the colonists had with tobacco, which enriched many people but fulfilled no mercantilist purpose.²⁵ One very great problem with all of the plans for growing new products was English lack of experience. Almost every scheme for importing plants from the Mediterranean or tropical countries was accompanied by plans to import skilled technicians to teach colonists how to grow and process the unfamiliar products. The colonists did not trust these foreign experts and hated being dependent on them.

It was also clear by the 1620s that even the southern part of North America was colder than expected from its latitude, which could not help but affect attempts to grow tropical crops. There was some confusion as well about just how hot the summer was. In London, William Crashaw wrote that, according to reports, Virginia was not so hot as countries in its corresponding European latitude, "for reasons not yet fully discerned"; although Virginia lies in the latitude of Toledo, the temperatures were more those of the south of France. Some writers said the summer climate was comparable to Spain's, but commentators noted that winters were sharper than anticipated, though no worse than those in England. If sharp, they were also short. And the extreme winter of 1607 was not seen as serious, because England had also experienced unusually cold temperatures that year.²⁶

Out of these circumstances, two contradictory trends developed simultaneously in Virginia. The first was the colonists' adaptation to their actual environment,

Brittania, in Force, *Tracts and Other Papers*, 1: 22; Kingsbury, *Records of the Virginia Company*, 1: 392; and Dunbar, *Geography of the North Carolina Outer Banks*, 8–9, 12–13, 113 n. 31, 117 n. 62.

²⁴ Joan Thirsk, ed., *The Agrarian History of England and Wales*, volume 4: 1500–1640 (Cambridge, 1967), 196; and Mea Allan, *The Tradescants: Their Plants, Gardens, and Museum, 1570–1662* (London, 1964), 54–55.

²⁵ Kingsbury, *Records of the Virginia Company*, 1: 392, 420, 510, 521, 623, 2: 32, 35, 102, 349–50, 384, 3: 166, 256–57, 303, 447–48, 471–76, 487, 647, 662–63, and 4: 4, 14–15, 23–24, 68, 125, 142, 163–64, 217, 230, 266–67, 452, 521–22, 583; H. R. McIlwaine, ed., *Minutes of the Council and General Court of Colonial Virginia, 1622–32, 1670–76* (Richmond, Va., 1924), 28; Virginia Company, *True Declaration of Estate*, 20, 23; and Dunbar, *Geography of the North Carolina Outer Banks*, 113 n. 31.

²⁶ Colt, "Voyage . . . to ye Ilands of ye Antilleas," 91–92; Capt. Wyatt, *Robert Dudley's Voyage to the West Indies, 1594–1595*, in George F. Warner, ed., *The Voyage of Robert Dudley to the West Indies, 1594–1595* (London, 1899), 13, 41, 53–54; William Strachey, *A True Reportory of the Wrack, and Redemption of Sir Thomas Gates Knight* (1610), in Purchas, *Hakluytus Posthumous: or, Purchas His Pilgrimes*, 19: 15–16, and *Historie of Travell into Virginia Britania*, 37–38; William Crashaw, *A Sermon Preached in London, before the Right Honourable the Lord Lawarre* (London, 1610), 35; Baron de la Warr, *The Relation*, 481; *A Relation of Maryland* (1634), ed. Francis L. Hawks (New York, 1865), 18; John Smith, *A Map of Virginia* (1612), in Barbour, *The Jamestown Voyages*, 2: 335, and *The Generall Historie of Virginia, New-England, and the Summer Isles* (1624), in Edward Arber and A. G. Bradley, eds., *The Travels and Works of Captain John Smith*, 2 vols. (Edinburgh, 1910), 1: 344, and 2: 632, 696; and Alexander Whitaker, *Good News from Virginia* (London, 1613), 39. Dunbar has noted that twentieth-century Williamsburg has a slightly higher average January temperature than London does, but it also has a greater range of temperatures; *Geography of the North Carolina Outer Banks*, 12.

adopting tobacco as the staple product, despite all manner of instructions and threats from England, and growing English grains and Indian corn for food. Enthusiastic reports of how well English grains prospered in Virginia belied bold talk of growing tropical fruits there. The colonists especially praised maize and the two harvests made possible each year by a combination of English and native grains.²⁷ The second trend was the obvious reluctance of all involved, both in England and in the New World, to accept the facts. Throughout the period, many of the same writers continued to assert that Virginia—in which they included everything from Roanoke to Maryland—would fulfill all of the expectations generated by its latitude.

In 1620 a Virginia Company publication told prospective investors, “Wee rest in great assurance, that this Countrey, as it is seated neere the midst of the world, betweene the extremitities of heate and cold; So it also participateth of the benefits of bothe, and is capable (being assisted with skill and industry) of the richest commodities of most parts of the Earth.” There followed a listing of the products of countries from Russia and Scandinavia to Persia, Italy, France, and Spain. Such assertions continued through the 1620s, and promotional pamphlets for the new colony of Maryland in the early 1630s were in the same tradition. Father Andrew White assured prospective colonists and investors that it was “probable that the soil will prove to be adapted to all the fruits of Italy, figs, pomegranates, oranges, olives, etc.” So, although many colonists resented investors’ pressure (“the manie wild and vast projects, set on foote all at one time, vizt 3 Iron Workes, saw mills planting of Silkegrasse, Vines, Mulberry trees &c. all of wch were enjoyned to be effected in the space of 2 yeares, by a handfull of men that were not able to build houses, and plant Corne to lodge and feed themselves, and so came to nothing”), promoters continued to base their expectations on the Virginia territory’s latitude.²⁸

THE PROBLEM OF ADAPTATION TO COLD CONDITIONS was far more serious in northern North America, and early experience was discouraging. Much of the best

²⁷ Kingsbury, *Records of the Virginia Company*, 1: 310, and 3: 30, 71, 73, 74, 455–56, 581; Samuel Purchas, “Virginian Affaires since the Yeere 1620, till This Present 1624,” in *Hakluytus Posthumous: or, Purchas His Pilgrimes*, 19: 146, 150. “Briefe Intelligence from Virginia,” *ibid.*, 209–10, and *Purchas His Pilgrimage*, 734; Strachey, *True Reportory of the Wrack*, 48, and *Historie of Travell to Virginia Britania*, 38; Archer, *Discription of the Now Discovered River and Country of Virginia*, 100–01; Hariot, *Briefe and True Report*, 337–39, 342–44, 386; Arthur Barlowe, *The First Voyage Made to the Coastes of America* (1584), in Quinn, *The Roanoke Voyages*, 1: 105–06; *Relation of Maryland*, 14, 27; Edward Waterhouse, *A Declaration of the State of the Colonie and Affaires in Virginia* (London, 1622), 4; Sir Thomas Dale to Mr. D. M., June 18, 1614, in Hamor, *True Discourse of the Present State of Virginia*, 55; John Rolfe, *A True Relation of the State of Virginia* (London, 1616), 35; Smith, *Generall Historie*, 541, 570; Father Andrew White, *An Account of the Colony of the Lord Baron of Baltimore in Maryland, Near Virginia* (1633), in C. C. Hall, ed., *Narratives of Early Maryland, 1633–1684* (New York, 1910), 10; Virginia Company, *A Declaration of the State of the Colonie and Affaires in Virginia* (1620), in Force, *Tracts and Other Papers*, 3: 4, and *True Declaration of Estate*, 12–13; and Patrick Copland, *Virginia’s God Be Thanked* (London, 1622), 13. In fact, of course, the colonists were unable, or unwilling, to feed themselves in the early decades of settlement. See, for example, Kingsbury, *Records of the Virginia Company*, 4: 174–82.

²⁸ Virginia Company, *Declaration of the State of Virginia*, 4; White, *Account of the Colony of . . . Maryland*, 10; Kingsbury, *Records of the Virginia Company*, 2: 527, and 4: 176, 178; *Relation of Maryland*, 25; Copland, *Virginia’s God Be Thanked*, 12–13; Waterhouse, *State of the Colonie and Affaires in Virginia*, 4–5, 31–32, 45–46; Purchas, “Virginia’s Verger,” in *Hakluytus Posthumous: or, Purchas His Pilgrimes*, 19: 242–43, 246, 251–52, and “Virginian Affaires,” 145–47, 150–53; and Smith, *Generall Historie*, 565.

information came from the French, who were on the scene early and were systematic in gathering and recording information. English promoters, such as Richard Hakluyt, were eager to learn from them and to see their writings published in English translations.²⁹ Jacques Cartier explored the St. Lawrence River in the 1530s. He found it colder than he expected, with floating ice thick in the harbors in May and ice one fathom and snow four feet deep from mid-November through mid-April. "The yce through all the Shippes, was above a hande-breath thicke, as well above hatches as beneath." But he found the heat greater than Spain's in July. Despite his experience, Cartier returned in 1541 with a group that attempted to found a colony near Quebec.³⁰

Samuel de Champlain was in what is now New England and Nova Scotia from 1604 to 1607. The first winter, spent in Maine, was harsh, with a very long snow cover. He reported that settlers should expect six months of winter, beginning in October; spring did not come until May. The cold, which was much more severe than in France, was so great that everything but the Spanish wine froze and "Cider was given out by the pound." He was puzzled by the discrepancy between actual and expected climate. Like Cartier, Champlain found the summers warm. Indians whom the explorers met on Cape Cod convinced them that winter there was mild, and the next two winters, spent in Nova Scotia, were actually much better than the first, leaving Champlain musing that perhaps the first had been a purely local phenomenon. Marc Lescarbot also wrote of the mild winter he experienced, but he said he knew other winters had been severe. He believed nature intended the long snow cover to act as a "fur-gowne" to protect the plants against excessive cold. Champlain returned, again like Cartier, bringing colonists to Quebec. He wrote that the winter there was shorter, and he and Lescarbot both believed that inland territories were milder. A mild winter in 1609–10 made Champlain very hopeful for the future of colonization there.³¹

During the same period, English voyages to the coasts of New England and the regions farther north also brought back mixed reports. Many people, frightened of southern heat, thought of New England as the place most suited to English settlers, the true sisterland to England. An early report by Bartholomew Gosnold, who moved along the coast and landed on Martha's Vineyard and Cape Cod during the summer of 1602, assured readers that New England, though colder than comparable latitudes in Europe, was warmer in winter than England. The seasons were

²⁹ Quinn, *England and the Discovery of America*, 315; and Douglas R. McManis, *European Impressions of the New England Coast, 1497–1620*, University of Chicago Department of Geography, Research Paper no. 139 (Chicago, 1972), 59–60, 137.

³⁰ Cartier, *A Shorte and Briefe Narration of the Two Navigations and Discoveries to the Northwest Partes Called Newe France*, trans. John Florio (London, 1580), 1–3, 18, 60, 66–67; John Alphonse, "Here Followeth the Course from Belle Isle, Carpoint, and the Grand Bay in Newfoundland up the River of Canada" (1542), in Richard Hakluyt, *The Principal Navigations, Voyages, Traffiques, and Discoveries of the English Nation*, 12 vols. (1598–1600; reprint edn., Glasgow, 1904), 8: 278; Sir Francis Roberval, *The Voyage of John Francis de la Roche, Knight, Lord of Roberval, to the Countries of Canada, Saguenai, and Hochelaga* (1542), *ibid.*, 286; and Carl O. Sauer, *Sixteenth-Century North America: The Land and the People as Seen by Europeans* (Berkeley and Los Angeles, 1971), 278.

³¹ Champlain, *The Voyages of the Sieur de Champlain* (1613), in H. P. Biggar, ed., *The Works of Samuel de Champlain*, 6 vols. (Toronto, 1922), 1: 302–07, 329, 352–53, 376–77, 446–47, and 2: 45, 57–58, 117–18; and Marc Lescarbot, *The Voyage of Monsieur de Monts into New France, Written by Marke Lescarbot*, in Charles Herbert Levermore, ed., *Forerunners and Competitors of the Pilgrims and Puritans*, 1 (Brooklyn, 1912): 282–83.

somewhat displaced: spring came later, but summer lasted longer in America than in England. Continuing confusion over climate in the north was generated by people like Gosnold, who wrote general statements about the climate after a visit of only a few months.³²

The year 1607 saw the foundation of two Virginia colonies, Jamestown by the London Company and Sagadahoc in Maine by the western merchants' Virginia Company. Partly because of bad management in the colony and reversals in England, but also because of the "extreame unseasonable and frostie" winter, Sagadahoc lasted less than a year. Raleigh Gilbert, who was in Maine, called it "that unseasonable Winter (fit to freeze the heart of a Plantation)." Reports emphasized that this winter was also extreme in Europe, with boats being built on the frozen Thames, but the reputation of an inhospitable climate stayed with northern North America. As Sir Ferdinando Gorges recalled in 1622, "all our former hopes were frozen to death," and more than a decade passed before anyone again tried to establish a colony in New England. John Smith said the country had been "esteemed as a cold, barren, mountainous, rocky Desart" after 1608.³³

Despite the failure at Sagadahoc, Sir Ferdinando, in his *Briefe Relation of the Discovery and Plantation of New England* (1622), argued that New England's latitude was in the center of the temperate zone, the "golden meane," and therefore comparable to "Constantinople, and Rome, the Ladies of the World; Italy, and France, the Gardens of Europe." He concluded the "Clime" had been proven to be temperate, delicate, and healthful "both by reason and experience." In his *Description of New England*, John Smith, reporting on his three-month voyage of 1614, asserted that New England's weather was cooler in both winter and summer than comparable latitudes in Europe and Asia. But he also predicted in a rhapsodical passage that commodities of all the richest areas of the world could be produced there. Smith did much to repair New England's public image after the Sagadahoc disaster, demonstrating that reasoning from latitude still appealed to the popular mind, especially among those without experience of the New England winter.³⁴

Settlers in New England during the 1620s expressed uncertainty and bewilderment about the weather. Plymouth colonists reported frost and snow in November and December of their first winter, 1620–21, and said they had heard the region

³² Gosnold, "Letter to His Father" (1602), in Purchas, *Hakluytus Posthumous: or, Purchas His Pilgrimes*, 18: 301. French explorers tried to avoid this mistake; see Champlain, *Voyages of the Sieur de Champlain*, 307, 352–53; and McManis, *European Impressions of the New England Coast*, 74.

³³ Charles Edward Banks, "New Documents Relating to the Popham Expedition, 1607," *Proceedings of the American Antiquarian Society*, 39 (1929), 318, 321, 324, 327, 330, 334; Gilbert, as quoted in Purchas, *Hakluytus Posthumous: or, Purchas His Pilgrimes*, 19: 296; Gorges, *Discovery and Plantation of New England*, 206–07, and *A Brief Narration of the Originall Undertakings of the Advancement of Plantations into the Parts of America* (1658), in Baxter, *Gorges and His Province of Maine*, 2: 16–17; Strachey, *Historie of Travell into Virginia Britania*, 173; and Smith, *Generall Historie*, 696. For the complex reasons for the colony's failure, see, in addition, Gorges to Sir Robert Cecil, in Baxter, *Gorges and His Province of Maine*, 3: 161; Richard A. Preston, *Gorges of Plymouth Fort* (Toronto, 1953), 141–50; and McManis, *European Impressions of the New England Coast*, 106–08, 137.

³⁴ Gorges, *Discovery and Plantation of New England*, 227–29, 231; John Smith, *A Description of New England* (1616), in Arber and Bradley, *Travels and Works*, 1: 196–98; and McManis, *European Impressions of the New England Coast*, 111–15. Also see George Popham to James I, December 13, 1607, in Henry O. Thayer, ed., *The Sagadahoc Colony* (Portland, Me., 1892), 118–19.

was both hotter and colder than England, although they had not yet experienced this. The next report from the Pilgrim leaders confirmed that the winters were colder but that they did not know why. The cold was mitigated somewhat, they said, by the longer winter days. Christopher Levett, a ship's captain who landed in New England in 1624 and spent several months there, believed the climate was as good as England's, but he was puzzled that it was not much better.³⁵

As settlements came to be longer-lived, writers from New England began to offer information about weather patterns rather than reports on a few seasons. William Wood wrote that the winter was cold but lasted no more than ten weeks. He quoted the Indians as saying that every ten years there was "little or no Winter," which he confirmed by his own experience of 1629–30, a mild winter, and by reports of the winter of 1620–21, which was also mild after an early cold period. Wood also stated unequivocally that the summers were hot. Wood forthrightly placed weather first in his book, and he declared that he preferred New England, summer and winter, to "the Summer Winters and Winter Summers of England." Francis Higginson wrote a glowing account of the possibilities of New England but was concerned to tell the truth about the weather. He said New England was colder than England in January and February and hotter in July and August. The two months of snow to be expected in winter appeared on his list of discommodities of the country. He reassured his readers that fuel was plentiful for the cold winters, a subject of concern in England, which had a serious wood shortage.³⁶ Finally, John Winthrop unflinchingly recorded information about the weather in his *Journal*, particularly noting bad winters and aberrations in summer temperatures. His data, covering the nineteen years from 1630 to 1649, prepared the way for a genuine analysis of the pattern of Massachusetts's weather. The winters of 1632–33 and 1637–38 were severe, and that of 1641–42 was worse. The bay was frozen from mid-January to late February, and people traveled over it in carts. Winthrop hastened to add, however, that that winter was harsh in Virginia as well; much of Chesapeake Bay was frozen. The hard winter was followed by a cold, wet summer in which some of the crops rotted in the fields. And the winter of 1645–46 was colder still.³⁷

The farther north the colony, the more serious the discussions of climate that emanated from it. During the early 1640s young Thomas Gorges was governor of

³⁵ Gosnold, "Letter to His Father," 301; William Bradford and Edward Winslow, *A Relation or Journall of the English Plantation Setled at Plimoth in New England*, known as *Mourt's Relation* (London, 1622), 8–21, 25–30, 62; William Bradford, *Of Plymouth Plantation, 1620–1647*, ed. Samuel Eliot Morison (New York, 1953), 70–71; Edward Winslow, *Good News from New England* (London, 1624), 62; Smith, *Description of New England*, 198; James Rosier, *A True Relation of the State of Virginia* (London, 1605), sig. e2; Levett, *A Voyage into New England* (London, 1624), 23, 26; and John Winthrop to John Winthrop, Jr., July 23, 1630, in Everett Emerson, *Letters from New England: The Massachusetts Bay Colony, 1629–1638* (Amherst, Mass., 1976), 50–51.

³⁶ Wood, *New Englands Prospect*, 3–8; and Higginson, *New Englands Plantation* (1631), in Force, *Tracts and Other Papers*, 1: 10–12. David M. Ludlum has interpreted Wood's remark as a reference to 1629–30. The winter of 1630–31 was cold; Ludlum, *Early American Winters, 1604–1820* (Boston, 1966), 12.

³⁷ John Winthrop, *Winthrop's Journal: History of New England, 1630–1649*, ed. James Kendall Hosmer, 2 vols. (New York, 1908), 1: 55–56, 58, 61, 69, 73–74, 89, 95, 98, 114, 119, 130, 137, 138, 143, 146, 157, 165–67, 223, 258, 269–72, 278–79, 291, 296, 306–07, and 2: 14, 44–45, 54–55, 57, 67, 81–82, 91–92, 126, 155–56, 224, 263–64, 267, 286, 328, 341, 346; Edmund Brown to Sir Simonds D'Ewes, September 7, 1638, in Emerson, *Letters from New England*, 228; Parry, *Climatic Change, Agriculture, and Settlement*, 163; and Ludlum, *Early American Winters, 1604–1820*, 6–15, 32–33.

another colony in Maine, this one established by his uncle, Sir Ferdinando Gorges. At the end of his first year there, he wrote that, although Maine was nearer the equator than England and therefore should be hotter in both summer and winter, this was not true. The summer heat was mitigated by cool winds off the ocean, although the first summer was hot enough to sour the beer. The winter was oppressive, especially the “most intollerable peircing winter” of 1641–42. “It is incredible to relate the extremity of the weather.” At other times he spoke of the dread of the approaching winter that suffused the colony. Thomas warned Sir Ferdinando that he could not expect the mills to run or the courts to meet during the winter, for “you must looke uppon us as prisoners from the end of 9ber till the beginning of Aprille.”³⁸

If New England’s winters were puzzling, Newfoundland’s cold was more serious, because people doubted if colonization was possible there. This led to massive accumulation of data and genuine efforts to understand the weather. The first expedition to investigate possibilities for colonization was that of Sir Humphrey Gilbert in 1583, a voyage that ended in his death at sea. Edward Hayes, who sailed with Gilbert, wrote a moving account of Sir Humphrey’s enthusiastic response to Newfoundland and of his death. Hayes noted that, as expected from its latitude, Newfoundland’s summer was hotter than England’s. Stephen Parmenius, who also died on the venture, wrote to his friend Richard Hakluyt that the summer sun was so intense the fishermen had to keep turning the fish on the drying racks to prevent their being spoiled or scorched. Both Hayes and Parmenius saw tokens of the extreme winter cold, especially the “huge masses of ice out to sea” in August. Parmenius saw “nothing but desolation” and fish in Newfoundland. Hayes was still enthusiastic, attempting to keep Gilbert’s venture alive despite his death; he admitted that northern Newfoundland was colder than it should be, since its latitude was that of Anjou, but affirmed that southern Newfoundland was sure to be as warm as expected.³⁹ By 1592, Hayes had shifted his attention to the promotion of New England and was arguing that Newfoundland’s climate was, after all, unsuitable; he denigrated Newfoundland in order to promote New England. Without having spent a winter there, he assured authorities that New England, being nearer the sun, must have winters one month shorter than those in England. He was also sure that it could not be so cold as some thought, since its latitude is 40 to 44 degrees. Thus, despite his experience and his shifts of opinion, Hayes continued to argue in the *a priori* mode, extrapolating from a model of the globe rather than from experience.⁴⁰

In 1610, a combine of merchants from London and Bristol sent a colony to

³⁸ Gorges, *Letters of Thomas Gorges, Deputy Governor of the Province of Maine, 1640–1643*, ed. Robert E. Moody (Portland, Me., 1978), 47, 49, 58, 87, 100–01, 112, 114, 116.

³⁹ Hayes, “An Abstract of This Discourse of Newfounde Lande” (1586), in Quinn *et al.*, *New American World*, 3: 125, “Consideracions to Induce Some Nobel Persons to Conquer and Possesse the New Founde Lande,” *ibid.*, 126, and *A Report of the Voyage by Sir Humfrey Gilbert, Knight* (1583), in Quinn, *Colonizing Enterprises of Sir Humphrey Gilbert*, 2: 405; and Parmenius to Hakluyt, 1583, in David B. Quinn and Neil M. Chesire, eds., *The New Found Land of Stephen Parmenius* (Toronto, 1972), 171, 173.

⁴⁰ Hayes and Carleill, “Discourse concerning a Voyage,” 158–60. This proposal met with a skeptical response in official circles; see Quinn *et al.*, *New American World*, 3: 172–75.

Cupid's Cove on Conception Bay in Newfoundland. They tried consciously to avoid the mistakes of the Virginia Company. They already had a valuable commodity, the fish of the Newfoundland Banks, to send back; moreover, they immediately moved to set up manufacturing works of various kinds. John Guy, the first governor, carefully selected the colony's site. The company ordered Guy to keep a journal of "all accidents and what wynds everie day and whether [weather] especially betweene September and march."⁴¹ Early reports made prospects look good. As the winter of 1610–11 approached, Guy wrote that the weather was superior to that in England and said he had heard it credibly reported that the only bad time for drift ice in the harbors was during the summer, when the fishermen were there. This long letter, full of the possibilities of the land, ends by stating that he would be able to write a more complete report "when the experience of the winter season is made." His letter to the company written after the first winter was published by Samuel Purchas; in it Guy claimed that fear of the climate was unnecessary, as he and his colonists had found the winter not so bad as many in England. Although the ponds were frozen for three months, the settlers had been able to travel all winter without hindrance. The months of October, November, and December, he said, were definitely better than in England.⁴²

The next several winters were a different story. Purchas received the weather journal kept by William Colston for the winter of 1611–12, which he declined to publish on the grounds that it would be "very tedious." He summarized the report in this way: "it appeareth that the weather was somewhat more intemperate than it had beene the year before, but not intolerable, nor perhaps so bad as we have it sometims in England." Henry Croute kept a meticulous weather journal for the period from September 1, 1612, to mid-May 1613; he took readings of wind direction, estimated temperatures, and described conditions in the morning, at noon, in the afternoon and early evening, and at several times throughout the night. A reliable thermometer had not yet been developed, but Croute distinguished among frost, hard frost, thawing, and heavy thawing as well as other degrees of mildness or warmth. The winter was long and hard, and the anguish of the settlers shows through, as in the entry for February 16, when moderate weather and thawing "putteth everyone in hope that winter is heer paste for this year." In fact, the last recorded snowfall was on May 12, and the journal ends abruptly in the middle of a sentence on May 13.⁴³ Although each month had days described as

⁴¹ Newfoundland Company, "Instructions Directed by the Counsaillae for the Plantation in Newfoundland to Iohn Guy to Be Observed in the Charge Recommended to Him" (1610), in Quinn *et al.*, *New American World*, 4: 141; and Gillian T. Cell, *English Enterprise in Newfoundland, 1577–1660* (Toronto, 1969), 54, 61–63. John Winthrop suggested to his scientist son that he should keep a weather diary to see the correspondence of old England and New; Winthrop to Winthrop, Jr., December 12, 1634, in Emerson, *Letters from New England*, 135–36.

⁴² Guy to Sir Percival Willoughby, October 6, 1610, in Quinn *et al.*, *New American World*, 4: 144–46, and to the Council of the Newfoundland Plantation, 1611, in Purchas, *Hakluytus Posthumous: or, Purchas His Pilgrimes*, 19: 410–12.

⁴³ "Journall from the First of September 1612 to the Last of Aprill 1613," in Quinn *et al.*, *New American World*, 4: 157–178, esp. 171, 178; and Purchas, *Hakluytus Posthumous: or, Purchas His Pilgrimes*, 19: 416. The addition of ten days is required to make the dates correspond to modern dates; thus, the date of the last snowfall corresponds to our May 22.

"very warme" and one in January was "so pleasaunce warme as though it had bin at mid sommer," Thomas Gorges's image of the colonists as wintertime prisoners runs through this journal and other reports. Repeated attempts to journey to other sites failed because snow made travel impossible. The bay and ponds were frozen many times, and the bay was invaded by "a verie hudg great Iland of ice." From February on, the journal is punctuated by reports of deaths from scurvy. The colonists knew that berries and grasses growing on Newfoundland could cure the sufferers, but attempts to gather them were unsuccessful. Not until the soil was workable and turnips left in the ground the previous fall could be dug up did the sick begin to recover.⁴⁴

After the initial years of settlement, the Newfoundland Company began to sell off parts of its patent to private investors, so sources of information became more varied in type and purpose, including more purely promotional pamphlets.⁴⁵ *Hakluytus Posthumous: or, Purchas His Pilgrimes*, including John Guy's letter and summaries of the weather journals, was published in 1625. During the 1620s, other writers were still claiming that the Newfoundland winter was not to be worried about. A promotional pamphlet published in Dublin proved the suitability of Newfoundland's climate for colonization by quoting Captain Wynne, "Gouvernor of Master Secretarie Calvert's plantation." Wynne's letter, written in 1622, said, "Neither was it so colde here the last Winter as in England the yeare before. I remember but three severall dayes of hard weather indeede, and they not extreame neyther: for I have knowne greater Frosts, and farre greater Snows in our owne Countrey."⁴⁶

So, even with more information available, the picture presented to the public was still mixed. But during the 1620s, in Newfoundland as in New England, some observers expressed their conviction that the weather was much colder than that in England. Sir William Vaughan, who never visited Newfoundland but was a prime mover of an attempt to found a new Wales there, and John Mason, who had taken over as governor at Cupid's Cove in 1616, tried to formulate rules by which the climate operates in Newfoundland. Both admitted that winter there was much colder than it was in comparable latitudes in Europe, although Mason claimed that summer temperatures were as expected. Vaughan compared Newfoundland to Yorkshire in winter, and Mason declared that places in America are as cold as the regions lying 12 to 13 degrees farther north in Europe. Mason, however, still clung to the belief that nature ought to be symmetrical, asserting that the same would be

⁴⁴ "Journall from the First of September 1612," 167, 174; and Croute to Sir Percival Willoughby, April 10, 1613, in R. A. Barakat, ed., *The Willoughby Papers: An Historical Record of Newfoundland's First Colony, 1610–c.1631* (forthcoming), 155–56, and "Description of a Voyage to Trinity Bay to Meet with the Natives" (1612), *ibid.*, 252, 254–56. I am greatly indebted to Dr. Barakat for allowing me to see his manuscript prior to publication. Sir William Vaughan publicized the colonists' discovery that turnips would cure scurvy, and he suggested that ships on long voyages carry turnips for ballast; Vaughan, *The Golden Fleece* (London, 1626), bk. 3: 45, 47–49, and *The Newlanders Cure*, 73.

⁴⁵ Cell, *English Enterprise in Newfoundland, 1577–1660*, 73.

⁴⁶ T. C., *A Short Discourse of the New-Found-Land* (Dublin, 1623), sig. B4. Similar praise of Newfoundland's climate as superior to England's occurs in a letter by Nicholas Hoskins of the same colony, which was printed in Richard Whitbourne's *Discourse and Discovery of New-Found-Land* (2d edn., London, 1622), 13, and in Robert Hayman's *Quodlibets, lately Come over from New Britaniola, Old New Found-Land* (London, 1628), 31.

true south of the equator. Both men believed that the cold was easier to bear in America than in England, because the days were longer in the winter. Emphasizing his three years and seven months in Newfoundland, Mason attacked earlier writers who had said that its air was better than England's, but he nevertheless found it "subtle and wholesome." Vaughan's attempts to establish colonies were short-lived; and by 1621 Mason, like Hayes before him, had shifted his attention to New England, where he became the founder of the colony of New Hampshire.⁴⁷

Sir George Calvert began his colony at Ferryland, Newfoundland, in the early 1620s. He went there briefly in 1627, after he resigned his position as secretary of state, and he had been created Baron Baltimore. In 1628, he returned to Newfoundland with his family, planning to remain. His "one intolerable wynter" there made him turn his back on that colony forever. He wrote to the king in August 1629 of his "too deare bought experience," and he claimed other men, for their "private interests," had concealed from him that

from the middest of October to the middlest of May there is a sadd face of wynter upon all this land, both sea and land so frozen for the greater part of the tyme, as they are not penetrable, no plant or vegetable thing appearing out of the earth, untill about the beginning of May, nor fish in the sea, besides the ayre so intolerable cold, as it is hardly to be endured. By means whereof and of much salt meate, my house hath been an hospitall all this wynter; of 100 persons, 50 sick at a time, myself being one; and nyne or ten of them dyed.⁴⁸

Vaughan claimed he had warned Lord Baltimore against choosing the very exposed site at Ferryland, "the coldest harbor of the Land," and was sure the colony would have succeeded had it been situated at the head of a bay in a sheltered spot. But, although some small groups of settlers remained in various colonies after 1630, and the fishing ships returned every spring, Newfoundland was now regarded as uninhabitable by English people.⁴⁹

FALLACIOUS ASSUMPTIONS ABOUT CLIMATE created disappointment and disillusionment great enough to threaten the entire colonial effort. People came to America inadequately prepared, physically and psychologically, to cope with the environment they actually encountered. This phenomenon continued long after regular contacts were established, because so many early expeditions, particularly fishing voyages to the Newfoundland Banks, came for the summer only. If the colonies were not to be seen merely as places for "a more regulated kind of killing of men,"

⁴⁷ Vaughan, *The Golden Fleece*, bk. 3: 22–26, 43; Mason, *Briefe Discourse*, sigs. A2v, A3v–A4, B2–B2v, B3; and Cell, *English Enterprise in Newfoundland, 1577–1660*, 77, 83–85.

⁴⁸ Calvert, Lord Baltimore, to Sir Francis Cottington, 1629, in Lawrence C. Wroth, "Tobacco or Codfish: Lord Baltimore Makes His Choice," *Bulletin of the New York Public Library*, 58 (1954): 527, and Baltimore to Charles I, August 19, 1629, in J. Thomas Scharf, *History of Maryland from the Earliest Period to the Present Day*, 1 (1879; reprint edn., Hatboro, Pa., 1967): 45; and Cell, *English Enterprise in Newfoundland, 1577–1660*, 92–95.

⁴⁹ Vaughan, *The Newlanders Cure*, 68–69. Also see the Reverend Joseph Mead's gossipy letter to Sir Martin Stuteville, January 23, 1629/30, in Thomas Birch, ed., *The Court and Times of Charles the First; Illustrated by Authentic and Confidential Letters . . .*, ed. [Robert F. Williams], 2 (London, 1848): 53.

an allegation reported by the London Virginia Company in 1621, promoters of colonization had to gather accurate information about the American climate and explain just how and why it differed from that of comparable latitudes in Europe.⁵⁰ Their efforts enable us to assess their understanding of climate and its causes, the ways in which the fruits of observation were assimilated, and the process by which theories were put forward and tested.

Writers from America were most impressive in their attempts to provide accurate information about climate. Frequently they corrected earlier reports, their own or others', often providing less favorable information in later reports. They also sought a framework in which to place the phenomena they observed. Some attempted to establish a mathematical formula to bring order to their data. Others left analysis to people at home, reporting their experience and deferring to "naturalists" or "astrologers" for interpretations.⁵¹ Dissatisfaction with available modes of explanation can be inferred from the use of several different, even contradictory, kinds. Early writers ultimately left the question open in such a way that hope remained for a better climate in the future.

One well-worn theoretical groove was an appeal to the wisdom of the ancients. But many writers referred to this source of understanding only to scoff at or refute classical knowledge, since the ancients knew nothing of the existence of America. Aristotle and his tradition had to be overcome to meet the objections of those timid people who continued to question the wisdom of transplantation to strange lands on the basis of the Aristotelian claim that the "burning" and the "frozen" zones were equally uninhabitable. Colonial enterprises in tropical America and the voyages of Sir Martin Frobisher and others to the far north had demonstrated that Aristotle's a priori reasoning was wrong.

The refutation of Aristotle had wide implications. In the battle of "ancients" versus "moderns," controversialists such as George Hakewill used the irrelevance of classical theories to the American experience to argue that the wisdom of the ancients was altogether inferior to modern knowledge. Many of the people who supported colonization and trade as a path to greatness for England were also involved in the movement to change the curriculum of the universities toward more practical subjects and away from the medieval tradition. Samuel Purchas pointed out that experience, not the study of the classics, provided the understanding required to make a success of overseas ventures. Aristotle, he said, had reasoned that the torrid zone would be extremely dry because the sun would burn away all of the moisture, whereas experience had demonstrated that the reverse was actually true.⁵²

⁵⁰ Virginia Company to the Governor and Council in Virginia, July 25, 1621, in Kingsbury, *Records of the Virginia Company*, 3: 489.

⁵¹ Bradford, *Of Plymouth Plantation, 1620–1647*, 302–03; Vaughan, *The Golden Fleece*, bk. 3: 23; and Gosnold, "Letter to His Father," 300–02.

⁵² Hakewill, *An Apologie or Declaration of the Power and Providence of God in the Government of the World* (Oxford, 1635), 282, 310; Purchas, *Purchas His Pilgrimage*, 718–21, 725–26; Charles Webster, *The Great Instauration: Science, Medicine, and Reform, 1626–1660* (London, 1975), 9, 19–21; and Karen Ordahl Kupperman, *Settling with the Indians: The Meeting of English and Indian Cultures in America, 1580–1640* (Totowa, N.J., 1980), 150–51.

George Best, who sailed with Frobisher, wrote a long refutation of the “olde writers persuaded by bare conjecture.” Best’s discourse, written in 1578, was an effort to fit experience into inherited frameworks of logical explanation and to disprove once and for all the classical idea that equatorial and far northern regions are incapable of supporting life. He admitted that topography—“accidents in the land”—can change an area’s climate, but he claimed that the greatest influences on climate are the angle of the sun’s rays and the length of time the sun is overhead. The error of the ancients was to reason from the sun’s angle alone. If both the angle and the duration are taken into account, Best argued, then equatorial regions can be proved to be not just habitable but the most delicate, fruitful, and commodious areas on earth. He was confident that the Garden of Eden lay on the equator. Equatorial regions are less hot in June than the tropics—or even France and England—because the sun is directly over the equator only two hours each year and the duration of the day is never more than twelve hours, so that much of the sun’s energy is spent in dispersing the mists and vapors left from the long nights. Because the sun moves from tropic to tropic over the equator, equatorial regions have two summers (in our spring and fall) and two winters (in December and June), “as may well appeare to him that hath onely tasted the principles of the Sphere.” Similarly, the northern regions, even at the North Pole, are also habitable, according to Best. Not only does the sun shine for a full six months there, but it is fully dark only a little more than a month in the winter. The long day is a period of “convenient, moderate, and temperate heat.” The floating mountains of ice that Frobisher encountered were to Best merely signs of summer warmth in the upper regions; that they had been detached from the glaciers demonstrated the melting power of the summer sun. Best’s arguments, dating from the earliest period of interest in colonization, show that even the proponents of experience over inherited wisdom were not clear about how to reason from experience alone or about how hypotheses should be tested.⁵³

Even after experience minimized the danger of the burning and frozen zones, some continued to fear them. Thomas Morton believed the tropics were habitable, but he agreed with Aristotle that the far north was too dangerous, pointing to the tragedies that had befallen some explorers there.⁵⁴ Other writers still feared equatorial regions. All of North America was habitable by the English, they argued, but Newfoundland was safest:

The journey is but easie to bee gonne,
The frozen Pole disjoyned farre doth lye:
We shape our course, farre from the burning Zonne,
The soile is subject to a milder skye.
And by prooffe, of many recordes tride:
The Paradise, of all the worlde beside.⁵⁵

⁵³ Best, *Experiences and Reasons of the Sphere, to Prove All Parties of the Worlde Habitable* (1578), in Hakluyt, *Principal Navigations*, 7: 252–83.

⁵⁴ Morton, *New English Canaan*, 13; and Hayes and Carleill, “Discourse concerning a Voyage,” 171–72.

⁵⁵ Bingham, “Maister Captaine Bingham, His Commendation uppon This Treatise,” preface to George Peckham, *A True Reporte of the Newfound Landes* (1583), in Quinn, *Colonising Enterprises of Sir Humphrey Gilbert*, 2: 439. Also see Hakluyt, the elder, *Inducements to the Liking of the Voyage Intended towards Virginia*, 328.

Another traditional form of explanation was to appeal to symmetry. Many educated Englishmen seem to have visualized the world as divided with geometrical precision—and with perfectly predictable characteristics for each division. According to this picture, 45 degrees of latitude, exactly mid-way between the equator and the North Pole, must have the most moderate temperatures. Since the extreme northern regions of New England lie at 45 degrees, some writers repeatedly referred to it as the “Golden Mean,” which enhanced its suitability for English settlers. Thomas Morton argued that New England was superior to the biblical Canaan, which lies at 30 degrees.⁵⁶ Edward Waterhouse said of Virginia that it was “near the midst of the world” and therefore was “betweene the extremities of heate and colde.” He printed a treatise by the mathematician Henry Briggs, who argued that Virginia’s middle position made it like a laboratory in which the products of all regions could be grown better than in their native soil.⁵⁷

Some colonists began very early to think about climate empirically. Others never did. Some writers mixed a priori and empirical answers, at times giving rigorous factual accounts of the weather they experienced and at other times appealing to symmetry. One writer appears to have intentionally misled prospective colonists, and he held an important place in the early promotion of colonization because his writing received official endorsement and its circulation was subsidized like no other. In parishes throughout England, the work of Richard Whitbourne, sea captain and promoter of Newfoundland, was distributed by order of the archbishops and Privy Council; the clergy were instructed to collect “voluntary contributions” to reimburse him for his work. Whitbourne merely said what was true, that Newfoundland was south of London, and estimated the distance variously as anywhere from 3 to 5 degrees of latitude, or 180 to 280 English miles. He simply invited the reader to draw his own conclusions. Whitbourne influenced people who did not themselves go to America but who wrote to encourage the emigration of others. Richard Eburne, for example, quoted Whitbourne with approval and repeated his reasoning about the relative latitudes of Newfoundland and London to demonstrate the hospitable nature of the American climate.⁵⁸

Whitbourne’s other tactic was to ridicule those who found Newfoundland’s climate uncomfortable, calling it “but a little needlesse charie nicenesse.” Colonists who suffered from the cold must “have been accustomed to drinke Tobacco, strong Ale, double Beere, or have beene accustomed to sit by a Taverne fire, or touched with the French disease.” Only the ignorant could find unpleasant a winter that he asserted the “Savages” endured naked and that was in any case less severe than many winters in England. Captain John Smith matched Whitbourne in his ridicule

⁵⁶ Gorges, *Discovery and Plantation of New England*, 228; Morton, *New English Canaan*, 14, 65; Smith, *Description of New England*, 206, *New Englands Trials* (1620), in Arber and Bradley, *Travels and Works*, 1: 253, and *Advertisements for the Unexperienced Planters of New-England, or Any Where* (1631), *ibid.*, 2: 938.

⁵⁷ Waterhouse, *State of the Colonie and Affaires of Virginia*, 3, 5; Briggs, *A Treatise of the Northwest Passage to the South Sea, through the Continent of Virginia and by Fretum Hudson*, *ibid.*, 45–46; and Crashaw, *A Sermon Preached in London*, sigs. E–EV.

⁵⁸ Whitbourne, *A Discourse and Discovery of New-Found-Land* (London, 1620), 1, 12, and *A Discourse containing a Loving Invitation* (London, 1622), sig. A4, p. 6; and Eburne, *A Plain Pathway to Plantations* (1624), ed. Louis B. Wright (Ithaca, N.Y., 1962), xi–xii, xvi–xvii, 136.

of the timid, although Smith in other places was rigorously honest about his experiences of weather in America. Smith and Whitbourne were self-made men who had risen in a world of privilege by enduring hardship and performing well in difficult situations. Smith was scornful of the “silly” people and “tender educats” who could not take the cold of New England, just as he was of the gentlemen of Jamestown who would not work even to save their own lives.⁵⁹

Some writers argued seriously that the colonists merely felt the winter's cold in America more than they did at home. Edward Winslow of Plymouth colony was disturbed by the inexplicable coldness of New England winters. He mused that perhaps the lack of the familiar comforts of home made the colonists feel cold. Sir William Alexander, writing from England, maintained that the colonists to whom Newfoundland had initially seemed cold now found it not much different from England.⁶⁰ This line of reasoning was attractive because it held out hope that the perception of America's climate as forbidding was a temporary phenomenon.

By the 1630s, thoughtful people involved in colonization realized that the north American winter was undeniably far colder than that of Europe. Since they assumed that Europe's oceanic climate was normal, America's deviation required explanation. William Bradford and John Winthrop offered a few examples of supernatural or catastrophic influences on climate. But most writers depended on natural and constant factors to explain America's climate. Although America is nearer the equator where the sun is overhead, some thought that the sun's rays hit the northern coast of North America “near at a right Angle, causing a strange reflection.” Others thought that the sun lost some of its heating power as it approached America over the ocean. Scientists believed that the sun drew vapors from the earth, which was generally beneficial; but over three thousand miles of ocean the sun was thought to draw up such large quantities of moisture into the atmosphere that its beams could not effectively penetrate to the land. Samuel Purchas wrote that the stars were smaller and fewer in the American sky, which made its climate colder.⁶¹

Some explorers noticed that there was a powerful current flowing northeastward from the Gulf of Mexico and were intrigued by it, but none formulated the idea that the current might warm Europe and that this might help account for the climatic differences between America and Europe. Mariners made use of the current, and Father White of Maryland believed it was responsible for the plentiful

⁵⁹ Whitbourne, *Discourse and Discovery of New-Found-Land* (1st edn.), 52–56; Smith, *Advertisements for the Unexperienced Planters*, 952; T. C., *Discourse of the New-Found-Land*, sig. B3v; and Anthony Parkhurst, “A Letter Containing a Report of the True State and Commodities of Newfoundland,” in Taylor, *Writings and Correspondence of the Two Richard Hakluyts*, 1: 131.

⁶⁰ Winslow, *Good News from New-England*, 62; and Alexander, Earl of Stirling, *The Mapp and Description of New-England*, known as *An Encouragement to Colonies* (London, 1630), 25.

⁶¹ Winthrop, *Journal*, 1: 270–71, 292, 306–07, and 2: 81–82, 224; Bradford, *Of Plymouth Plantation, 1620–1647*, 131, 302–03; Mason, *Briefe Discourse*, sig. B2v; Best, *Experiences and Reasons of the Sphere*, 255–56; Hayes, *Report of the Voyage of Sir Humphrey Gilbert*, 405; Hayes and Carleill, “Discourse concerning a Voyage,” 158–60, and *Treatise Containing Important Inducements for Planting*, 15; Gorges, *Discovery and Plantation of New England*, 228–29; Ligon, *True and Exact History of the Island of Barbados*, 28; Morton, *New English Canaan*, 14; Lescarbot, *Voyage . . . into New France*, 282; Purchas, *Purchas His Pilgrimage* (2d edn.), 734; Lamb, *Climate: Present, Past, and Future*, 1: 31, and *The Changing Climate*, 141; and Wright, *Geographical Lore of the Time of the Crusades*, 22.

supply of fish off the coast of America. Gabriel Archer hoped that “subtle invention” would discover the “true cause thereof.”⁶² The action of freshwater ponds and rivers was interpreted differently depending on the area. Samuel Purchas argued that an abundance of lakes and rivers moderated the climate of Virginia, making it less hot than feared. In the north, though, the wind blowing across frozen ponds and lakes was seen as one reason for extreme cold. The large number of lakes and ponds created the underlying conditions for cold weather, wrote John Mason of Newfoundland, since “fresh waters” are “more naturally cold than salt, and both colder than the earth.”⁶³

By far the most popular explanation of cold winter temperatures—an explanation advanced by every writer on Newfoundland—centered on the huge masses of floating ice in the ocean and bays. Stephen Parmenius left a graphic description of this phenomenon:

Some of our company have reported that in the month of May they were stuck for 16 whole days on end in so much ice that some of the icebergs were 60 fathoms thick; and when their sides facing the sun melted, the entire mass was turned over, as it were on a sort of pivot, in such a way that what had previously been facing upwards was then facing down, to the great danger of any people at hand, as you can well imagine.

These “Monsters” were said to create colder temperatures as winds blew across them, and their presence made colonists feel more cold and miserable than they otherwise would have. Newfoundland colonists liked this explanation because it made sense of their own experience and enabled them to argue that Newfoundland was not naturally cold. They looked upon the icebergs as “accidentall” and not natural to the area, having floated in from the frozen north.⁶⁴ During the little ice age there was much more floating ice in the North Atlantic than there has been in the twentieth century, and it was thick much farther south—Iceland and Greenland were sometimes “besieged” by ice. Modern climatologists agree with seventeenth-century writers that such sea ice can drastically change the climate of surrounding land areas, altering the heating pattern of the overlying atmosphere and the course of the prevailing winds.⁶⁵

⁶² White, *Account of the Colony of . . . Maryland*, 8; Archer, “The Relation of Captaine Gosnols Voyage to the North Part of Virginia” (1602), in Purchas, *Hakluytus Posthumous: or, Purchas His Pilgrimes*, 18: 303; Samuel Argall, *The Voyage of Captaine Samuel Argall* (1610), *ibid.*, 19: 77; Abram Kendall, “Ruttier by the Learned Mariner Abram Kendal, Englishman,” in Warner, *The Voyage of Robert Dudley*, 90; and Hayes, *Report of the Voyage of Sir Humfrey Gilbert*, 391–92. For changing opinions of the role of the Gulf Stream, see Chapin and Walton Smith, *The Ocean River*, chaps. 6–7; and Stommel, *The Gulf Stream*, 173–76.

⁶³ Purchas, *Purchas His Pilgrimage* (2d edn.), 720; Gorges to Henry Gorges, in Gorges, *Letters*, 47; Mason, *Briefe Discourse*, sig. b2v; and Alphonse, “Belle Isle, Carpont, and the Grand Bay in New Foundland,” 281.

⁶⁴ Parmenius to Hakluyt, 173; “Iournall from the First of September 1612,” 174; Richard Hakluyt, *The Voyage of M. Hore and Divers Other Gentlemen, to Newfoundland, and Cape Briton, in the Yere 1536* (1589), in Taylor, *Writings and Correspondence of the Two Richard Hakluyts*, 395; Mason, *Briefe Discourse*, sigs. A3v, b2v–b3; T. C., *Discourse of the New-Found-Land*, sig. b3v; Vaughan, *The Newlanders Cure*, 68–69, and *The Golden Fleece*, bk. 3: 23; Hayes and Carleill, “Discourse concerning a Voyage,” 162; Hayes, *Report of the Voyage of Sir Humfrey Gilbert*, 397; Whitbourne, *Discourse and Discovery of New-Found-Land* (1st edn.), 55–56; Parkhurst, “Report of the True State and Commodities of Newfoundland,” 131; Purchas, *Hakluytus Posthumous: or, Purchas His Pilgrimes*, 19: 418–19; Best, *Experiences and Reasons of the Sphere*, 276–82; and Sauer, *Sixteenth-Century North America*, 246.

⁶⁵ Lamb, *Climate: Present, Past, and Future*, 1: 86, 385–86; Parry, *Climatic Change, Agriculture, and Settlement*, 30; Calder, *The Weather Machine*, 15, 70–71; and E. G. R. Taylor, *Late Tudor and Early Stuart Geography, 1583–1650* (London, 1934), 22.

No explanation was completely satisfactory to early modern people. John Mason, for example, who wrote some of the most detailed reports of actual weather he experienced as well as thoughtful analyses of its causes, advanced all three of the most popular explanations: the sun's drawing of humors and the loss of penetrating power in its advance over water; the strange angle of the sun's rays; and the floating mountains of ice. At the outset, he said he would give his reasons "according to mine opinion (submitting my selfe to better Judgements)." Such openmindedness was associated with a willingness to suspend preconceptions and record observations accurately. Such mixed explanations for climatic variation were not unusual. Sometimes, writers who presented theories based on experience also resorted to the old theory of natural symmetry. Many remained puzzled.⁶⁶

FEW OF THOSE INVOLVED IN COLONIZATION were content to end with an acknowledgment and explanation of cold. What did it mean for the future of English America? Promoters argued that none of this new information meant that English settlers could not succeed in North America; they confidently asserted that colonists could adjust to unfamiliar conditions of heat and cold and make even forbidding-sounding places fruitful. "Hee is wretched that beleeves himself wretched." Several promoters, mindful of the belief that each climate produces its own unique balance of the humors, asserted that all of North America was compatible with English bodies. The perfect agreement of English constitutions and American air was even urged as proof that God had intended North America for the English nation. At worst, the winters would be like those in Holland; and, in any case, only Spaniards feared the cold, not the tough and flexible English. Moreover, fuel was so plentiful in America that colonists could "have Christmas fires all winter," and they would have the comfort of the winter sun's rays much longer each day than in England's far northern latitude.⁶⁷

Philip Vincent reminded his readers, "All beginnings are ever difficult." After gaining some experience, however, many colonists complained bitterly of those who had earlier written "somewhat hyperbolically" and misled colonists about the hard work necessary to establish settlements. Some people now went to the opposite extreme and declared the country barren. The unfit were warned not to come; and many writers noted with satisfaction that those unequal to the initial hardships were leaving. Extravagant praise of the country in early writings also produced a kind of moral revulsion on the part of colonists. That people had been led to believe they could pick up necessities and riches without labor offended both common sense and contemporary ideas of the morally sound life. Human beings are not meant to

⁶⁶ Crashaw, *A Sermon Preached in London*, 35; and Mason, *Briefe Discourse*, sigs. A3v, b2–b3.

⁶⁷ George Donne, *Virginia Revived* (1638), ed. T. H. Breen, *William and Mary Quarterly*, 3d ser., 30 (1973): 463; Vincent, *True Relation of the Late Battel*, sig. b, p. 19; Eburne, *A Plain Pathway to Plantations*, 47–48; Vaughan, *The Golden Fleece*, bk. 3: 14, 23–26; Mason, *Briefe Discourse*, sig. b2; Thomas Welde to Parishioners at Tarling, 1632, in Emerson, *Letters from New England*, 96; Jackson, *Voyage . . . to the Western Indies*, 30; Virginia Company, *True Declaration of Estate*, 12; Higginson, *New Englands Plantation*, 11; John White, *The Planters Plea* (1630), in Force, *Tracts and Other Papers*, 2: 29; Wood, *New Englands Prospect*, 8; Smith, *Generall Historie*, 627, and *Advertisements for the Unexperienced Planters*, 952–53; and Winslow, *Good News from New England*, 52.

live idly, "when Adam himselfe might not live in paradise without dressing the garden."⁶⁸ Later reports stressed that people would be able to live comfortably in America, but not without effort. The English believed that nature as created by God represents pure potential and that humanity, God's agent, brings all to its highest realization in the Old World as well as the New. John Guy wrote that America lacked nothing to make it "a flourishing Country but cattle and the industry of man; without which our Country [word missing] be as bad as this." He went on to say that God "hath ordayned that the earth of it selfe should not be fruitfull without the sweate of mans browes." Although a common attitude among later colonists, this sober assessment was probably most congenial to the Puritan founders of Plymouth and Massachusetts Bay. They stressed that people should not expect ever to have more than a "sufficiency." One effect was to discourage "strangers" from trying to settle among them. The Adventurers supporting Plymouth wrote to the colonists in this vein: "If your place be not the best, it is better; you shall be less envied and encroached upon; and such as are earthly minded will not settle too near your border."⁶⁹

Writers stressing the need for reasonable expectations also ridiculed those who dropped their normal, common sense standards when they looked at America. One favorite theme was that America was rich in fish and all kinds of plant and animal life—but only in the proper seasons. John Smith answered those who said the country was barren by claiming they were as absurd as people trying to pick cherries in Kent at Christmastime. That none could be picked in winter did not mean that Kent did not produce cherries. George Best pointed to failed experiments with orange trees in England, from which no one had concluded that England was uninhabitable. Another aspect of this new mood of reasonableness was the admission that not all parts of America were equally desirable. If America had its foggy and marshy or hilly and rocky areas, so did England. America should not be judged by its unhealthy places any more than England by the fens of East Anglia, Kent, or Essex. The mosquitos, of which almost everyone in the northern colonies complained, were one of those "discommodities" that would disappear with increased cultivation and habitation of the land.⁷⁰ This comparison had great

⁶⁸ Vincent, *True Relation of the Late Battell*, sig. B4; Wood, *New Englands Prospect*, 47–48; Thomas Dudley to the Countess of Lincoln, 1630/31, in Emerson, *Letters from New England*, 72, 75; Francis Higginson to Friends at Leicester, 1629, *ibid.*, 26–27; Winthrop to Winthrop, Jr., 50, and *Journal*, 1: 333; Mason, *Briefe Discourse*, sigs. A2v, B3v; Kingsbury, *Records of the Virginia Company*, 4: 161; Virginia Company, *True Declaration of Estate*, 15; and Winslow, *Good Newes from New England*, 65–66. Joseph Duncan has pictured John Milton's Adam needing a chain saw to keep up with the work required of him; Duncan, *Milton's Earthly Paradise: A Historical Study of Eden* (Minneapolis, 1972), 159, 238–39.

⁶⁹ Guy to Willoughby, 145; Alexander, *An Encouragement to Colonies*, 26–27, 43; Hakewill, *Declaration of the Power and Providence of God*, 98–100; Robert Gordon of Lochinvar, *Encouragements to Under-Takers* (Edinburgh, 1625), sigs. D4, E2v; Bradford and Winslow, *Mourt's Relation*, 61–62; Peckham, *True Reporte of the Newfoundland Landes*, 468; Bradford, *Of Plymouth Plantation, 1620–1647*, 129; Robert Cushman, "Epistle Dedicatory" to *A Sermon Preached in Plimmoth in New England* (1622; reprint edn., New York, 1847), xii; Winslow, *Good Newes from New England*, 64–65; Morton, *New English Canaan*, 5–6; White, *The Planters Plea*, 32–33; Richard Mather to William Rathband and Mr. T., 1636, in Emerson, *Letters from New England*, 205; and Quentin Skinner, *The Foundations of Modern Political Thought*, 1 (Cambridge, 1978): 98–99.

⁷⁰ Smith, *Description of New England*, 198–208. *New Englands Trials*, 264, and *Advertisements for the Unexperienced Planters*, 930–38; Best, *Experiences and Reasons of the Sphere*, 270; Higginson, *New Englands Plantation*, 8–9, 11; Winslow, *Good Newes from New England*, 12; Bradford and Winslow, *Mourt's Relation*, 2: 21, 62; Archer,

force in the early seventeenth century, when England's fens and mosquitos created the necessary conditions for endemic malaria. Oliver Cromwell is said to have died of English malaria, contracted during his early life in East Anglia. Sir William Vaughan also argued that even within England different regions on the same parallel have different climates, citing what he considered to be the superior climate of Wales over that of London and Essex.⁷¹ Since all lands have some barren or difficult areas, English promoters should not have expected America to be different.

Slowly the burden of blame was shifted from the country to the people; that America had not satisfied early expectations was the fault of the inhabitants—Indians and colonists—who had shirked the God-given task of development, leaving North America “onely as God made it, when he created the worlde.” Similarly situated countries in Europe, according to John Smith, were “beautified by the long labour and diligence of industrious people and Art.” European technology and industriousness would produce sweeping changes in the natural environment, and America might still satisfy many of the original expectations. Samuel Purchas maintained that “the good things of Virginia are naturall and her owne, the bad accidentall and our owne.”⁷²

Not only could the English adjust psychologically and physically, but, it was argued, they could also adjust technologically. If the land did not offer the expected products, they would work hard and discover what commodities it would produce. All of the early colonies were initially dependent on maize for their staple diet, and many settlers praised it unstintingly, seeing it as superior to English grains in that, its “natural” climate. Colonists in every region also reported that they were experimenting with English grains and keeping records of planting and harvesting times, types of soil, and which grains thrived. The discouraged were reminded that Poland was one of the great grain-producing areas of Europe and that much of Europe's population lived well in “Muscovia, Sweidon, Norway, Spruceland, Poland, Denmarke, and other Easterne and Northern parts of the world.”⁷³

A major shift also had to be made in the commodities the colonists were expected to send back to England to recompense investors. Again, the question was subtly

Description of the Now Discovered River and Country of Virginia, 99; Mason, *Briefe Discourse*, sigs. A2v, A4v–B; Donne, *Virginia Reviewed*, 455; Whitaker, *Good News from Virginia*, 42; William Morrell, *New England* (London, 1625), 15; Gorges, *Letters*, 58; Winthrop, *Journal*, 1: 89; White, *The Planters Plea*, 29–30; Virginia Company, *True Declaration of Estate*, 14; Bradford, *Of Plymouth Plantation, 1620–1647*, 144; Emerson, *Letters from New England*, 215; Browne to D'Ewes, 228; Levett, *A Voyage into New England*, 25; Kingsbury, *Records of the Virginia Company*, 3: 304; Wood, *New Englands Prospect*, 46–47; and Strachey, *Historie of Travell into Virginia Britania*, 70, and *True Reportory of the Wrack*, 59.

⁷¹ Vaughan, *The Newlanders Cure*, 69–70; Strachey, *True Reportory of the Wrack*, 58; Virginia Company, *True Declaration of Estate*, 14; Smith, *Description of New England*, 204; Mary Dobson, “‘Marsh Fever’—The Geography of Malaria in England,” *Journal of Historical Geography*, 6 (1980): 357–89; and Lamb, *The Changing Climate*, 41.

⁷² Smith, *Description of New England*, 197, and *The Proceedings of the English Colonie in Virginia*, known as *Map of Virginia, Part II* (1612), in Barbour, *The Jamestown Voyages*, 2: 438; Gordon of Lochinvar, *Encouragements to Under-Takers*, sig. c3v; and Purchas, *Hakluytus Posthumous; or, Purchas His Pilgrimes*, 19: 211. Smith, who many times admitted that he and his men lived off the products of Indian agriculture, knew that in rhetorical flourishes such as these he was falsifying the record.

⁷³ Whitbourne, *Discourse and Discovery of New-Found-Land*, 52–53. For a full description of colonists' opinions of maize, see Kupperman, *Settling with the Indians*, 82–85.

transformed into a moral as well as a practical one. Mediterranean people had gone into places where gold and silver were to be had without effort, but this path was corrupting. Honest labor and honest commodities were better for English people. The fish, furs, tobacco, and wood products that English labor could produce in North America would be superior to quick and easy wealth, which undermines the character of the people and the foundations of society. Edward Hayes argued that nature had made northern North America especially abundant as a recompense for "some sharpe cold" in its climate. John Smith hammered constantly on the theme that Holland was a richer, more powerful, and greater country than Spain and that Dutch wealth was derived from "this contemptible trade of fish." England must copy the Dutch, not the Spanish, if it was to become a great nation. As Edward Waterhouse forcefully pointed out, gold and silver run out but the products of honest labor do not.⁷⁴

The cold of North America could even be seen as advantageous in trade. William Strachey reminded his readers that England bought commodities from northern as well as southern Europe; to obtain northern products from their own colonies would free capital to buy southern products more easily. Edward Hayes argued that in this way the fish of New England could be transformed into "Wines, Sweet oiles, Fruits, Spices, Sugars, Silks, Gold and Silver, or whatsoever Europe yieldeth, to supply our necessities, and to increase our delights." If English North America was as cold as people had come to believe, then the "naked" savages, as well as future English colonists, would be a major market for English woolen cloth, which would enrich the nation. God did not design nations to be totally self-sufficient; a cooperative relationship was necessitated by divine plan. Samuel Purchas rhapsodized about the "compleat commerce, in venting . . . the principall of English Commodities, Cloth and Wooll, with the Gospel of our Lord Jesus."⁷⁵

So promoters of colonization in America were not without high hopes as they surveyed the environment. Most, though, continued to believe that the products of southern Europe could eventually be grown in English North America because they believed that in the future the climate would become warmer and less given to extremes. Their original hopes were seen as postponed rather than given up altogether.

One line of reasoning was that only the coast of America was wild in its weather and that the climate further inland would correspond to original expectations. William Strachey pictured Virginia as the "suburbs" of the great American continent, and Purchas saw the forbidding quality of the coast as part of Virginia's maidenly modesty. Several used an organic image, arguing that the "bowels," the

⁷⁴ Hayes, *Report of the Voyage of Sir Humfrey Gilbert*, 406–08; Hayes and Carleill, "Discourse concerning a Voyage," chap. 5, 163–65; Smith, *Description of New England*, 194, *New Englands Trials*, 253–55, *Generall Historie*, 784, and *Advertisements for the Unexperienced Planters*, 944–45; and Waterhouse, *State of the Colonie and Affaires in Virginia*, 31–32.

⁷⁵ Hayes and Carleill, *Treatise Conteyning Important Inducements for Planting*, 17–18; Strachey, *Historie of Travell into Virginia Britania*, 21; George Benson, *A Sermon Preached at Paules Crosse* (London, 1609), 26; and Purchas, "Virginia's Verger," 251–52. Also see Virginia Company, *True Declaration of Estate*, 25; Carleill, *Briefe and Summary Discourse*, 357–58; Lane, "Letter from Virginia," 209; and Hakluyt, *Discourse of Western Planting*, 235.

"heart and intralls" of the country would be richer and quite different from the periphery.⁷⁶ Some observers presented reasons for this expectation; others simply asserted the interior was richer and more temperate. Thomas Hariot of Roanoke extrapolated from the soil, growth patterns of trees and grasses, and Indian settlements he had seen on the mainland when he said the interior was better in every way, but his claims went beyond his experience. Edward Hayes reasoned that the inland portions of Newfoundland would be warmer because they were protected by mountains from the winds. Conversely, James Rosier argued that New England beyond the coast was not cut off from the south by mountains and would be warmer. Richard Ligon of Barbados simply asserted that a continent must have more settled weather and drier and purer air than a small island. Most writers felt they could safely assume the superiority of inland territories because of their assumptions about the causes of America's coldness. The effect of the sun's approach over water and of the floating ice in the sea would be mitigated in areas farther to the west. All might still be as originally expected.⁷⁷

The other major line of reasoning that preserved hope for the future productiveness of America centered on the colonists' ideas of the effect of human labor on the environment. Colonists firmly believed that the climate of America, under the impact of settlement by Europeans with their agricultural technology, would become healthier, warmer, and more temperate. Nature cannot reach its full potential without human care; it may even degenerate without it:

The healthfulness of any country by plantation and inhabitation must needs be much increased. For, the ridding of ground, casting of ditches and watercourses, and making of fires, together with the destroying of wild and filthy beasts, all which and other like do necessarily accompany any good plantation, further much to the cleansing of the air, clearing of fogs, and so ridding of much corruption and unhealthiness from the place.

Richard Whitbourne believed that cutting down the woods of Newfoundland would clear vapors and raise temperatures by allowing the sun's rays to penetrate more effectively and be retained. John Mason said uncultivated lands are naturally colder; moreover, he maintained, Newfoundland was cooler than England because it lacked the fires of people's houses and the warmth radiated by towns and cities. William Wood reported that rainfall patterns in Massachusetts Bay were changing

⁷⁶ Strachey, *Historie of Travell into Virginia Britania*, 34, 40, 118; Purchas, *Purchas His Pilgrimage*, 748 (misnumbered; actually 754); Hayes and Carleill, "Discourse concerning a Voyage," 167, and *Treatise Conteyning Important Inducements for Planting*, 20; Virginia Company, *A True and Sincere Declaration of the Purpose and End of the Plantation Begun in Virginia* (London, 1610), 18; and Smith, *Description of New England*, 197. For the persistence of this idea, see Harry Roy Merrens, *Colonial North Carolina in the Eighteenth Century: A Study in Historical Geography* (Chapel Hill, 1964), 35, 44, and "The Physical Environment of Early America: Images and Image Makers in Colonial South Carolina," *Geographical Review*, 59 (1969): 541–42, 550–51.

⁷⁷ Hariot, *Briefe and True Report*, 382; Hayes, *Report of the Voyage of Sir Humphrey Gilbert*, 405, and "Consideracions to Induce Some Noble Persons to Conquer and Possesse the New Founde Lande," 126; Rosier, *True Relation of the State of Virginia*, sig. d2; Ligon, *True and Exact History of the Island of Barbados*, 86; Parkhurst, "Report of the True State and Commodities of Newfoundland," 131; Richard Hakluyt, "Epistle Dedicatory" to Hakluyt, *Principal Navigations* (1599), in Taylor, *Writings and Correspondence of the Two Richard Hakluyts*, 2: 456; Gorges, *Discovery and Plantation of New England*, 228; Vaughan, *The Golden Fleece*, bk. 3: 27–29; Alexander, *An Encouragement to Colonies*, 34; Eburne, *A Plain Pathway to Plantations*, 48; and Peckham, *True Reporte of the Newfoundland Landes*, 467.

even within his own experience there and that England's weather had formerly been like that of New England.⁷⁸

AMERICA'S FAILINGS WERE, AFTER ALL, HUMAN FAILINGS. And they could be rectified by human activity.⁷⁹ If England could just support the colonies through their initial period of hardships and low returns, then all hopes would eventually be realized. Throughout the early period of colonization, there was continuing tension between hope that the environment might meet English expectations and requirements and accommodation by English settlers to the environment actually encountered. The steady accumulation and assimilation of facts about the climate of eastern North America threatened the classical concept of climates—the belief that climate is constant in any latitude around the world. Yet colonists and promoters struggled to adapt the old concept to fit new evidence, or to interpret the evidence in such a way that the old paradigm could be saved. In the absence of instrumental data, people could continue to believe that the contrast between the English and American climates was partly a function of perceptions. The rate of assimilation of the evidence varied from region to region; it was greater where the discrepancy between expectations and reality was most pronounced. Newfoundland came to be rejected for colonization, although its importance as a fishing ground continued. New England was finally perceived as a rough country where settlement was possible but whose products would rival those of northern, not southern, Europe. In the early seventeenth-century southern mainland colonies, settlers continued to

⁷⁸ Eburne, *A Plain Pathway to Plantations*, 48; Hakewill, *Declaration of the Power and Providence of God*, 156–57; Purchas, *Hakhytus Posthumous: or, Purchas His Pilgrimes*, 20: 134; Vaughan, *The Golden Fleece*, bk. 3: 27–29; Whitbourne, *Discourse and Discovery of New-Found-Land*, 57; Mason, *Briefe Discourse*, sig. b2v; and Wood, *New Englands Prospect*, 7–8. French colonists also believed North America would become warmer with cultivation; see Champlain, *Voyages of the Sieur de Champlain*, 303; Lescarbot, *Voyage . . . into New France*, 283; and Alphonse, “Belle Isle, Carpont, and the Grand Bay in New Foundland,” 282–83. New England colonists received powerful reinforcement for this idea in the 1650s and 1660s, decades that saw a trend toward warmer winters; see Karen Ordahl Kupperman, “Climate and Mastery of the Wilderness in Seventeenth-Century New England,” paper presented at the Colonial Society of Massachusetts Conference on Seventeenth-Century New England, held in Boston, June 1982, and forthcoming in David D. Hall and David Grayson Allen, eds., *New Perspectives in Seventeenth-Century New England History*. For discussion of the actual effect of forest clearance on cultivation, see Lamb, *Climate: Present, Past, and Future*, 1: 7, 50; Tickell, *Climatic Change and World Affairs*, 26; Oliver, *Climate and Man's Environment*, 164–68; and Alexander T. Wilson, “Isotope Evidence for Past Climatic and Environmental Change,” *Journal of Interdisciplinary History*, 10 (1980): 810–12.

⁷⁹ The belief that humanity has the power and duty to manipulate the environment was still very much alive in the late eighteenth century. Thomas Jefferson answered the *philosophes'* assertion that America was an inferior continent by arguing that the environment here, and especially the climate, was changing noticeably year by year under the impact of European-style cultivation: both heat and cold were becoming more moderate. In the nineteenth century, scientists and laymen endorsed the claim that the climate of the American West could be changed by cultivation, a position that was summed up in the slogan, “Rain follows the plow.” We still believe today that human activities have enormous influence on the climate, both long- and short-term, as the concern over carbon dioxide in the atmosphere and the warming effect it will have shows. What has changed is that we are less sanguine than sixteenth- and seventeenth-century observers that such effects are always beneficial. Jefferson, *Notes on the State of Virginia* (1785), ed. Thomas Perkins Abernethy (New York, 1964), 42–44, 75, 79; Gilbert Chinard, “Eighteenth-Century Theories on America as a Human Habitat,” *Proceedings of the American Philosophical Society*, 91 (1947): 25–57; Antonello Gerbi, *The Dispute of the New World: The History of a Polemic, 1750–1900*, trans. Jeremy Moyle (Pittsburgh, 1973); Henry Steele Commager and Elmo Giordanetti, *Was America a Mistake? An Eighteenth-Century Controversy* (New York, 1967); and W. Eugene Hollon, *The Great American Desert, Then and Now* (New York, 1966), chap. 9.

base their expectations on latitude and to hope they could eventually produce commodities comparable to those England imported from southern Europe.⁸⁰

By the middle of the century, most people involved in colonization were still convinced that European technology and English industriousness would transform the American environment beyond recognition. With their faces turned toward the west, the interior that, they were convinced, would be better and more like the paradigmatic climate, settlers and promoters struggled to hold their varying, often contradictory, beliefs simultaneously in their minds.

⁸⁰ For the revival of these claims at mid-century, see W. F. Craven, *White, Red, and Black: The Seventeenth-Century Virginian* (Charlottesville, Va., 1971), 21–22.

AHR Forum
Social Structure and Politics
in American History

EDWARD PESSEN

ONE OF THE GREAT NEGLECTED THEMES in American historical thought is the interrelationship between politics and the social or class structure over the course of American history.¹ Brilliant works have, of course, been written on American political history without referring to this interrelationship. But, in bypassing it, the authors of even the most illuminating studies have ignored a vital dimension of politics.² One need not agree completely with Jackson Turner Main's contention that "an understanding of political history depends upon mastery of the underlying social structure"³ to believe that an improved understanding, if not necessarily "mastery," of the social structure of any community is likely to enhance understanding of its politics.

A division of labor appears to have obtained among American political historians, in which iconoclasts or those preoccupied with the seamy side of the "social reality" almost alone pay close attention to that reality. What is disquieting is the evident widespread assumption that close analysis of the reciprocity between politics and

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¹ The term "social structure," as sociologist Peter M. Blau has recently observed, is "used widely in sociology, . . . often broadly, and with a variety of meanings"; Blau, "Parameters of Social Structure," in Blau, ed., *Approaches to the Study of Social Structure* (New York, 1975), 220. In using the term as though it were interchangeable with class structure, I am following Harold Lasswell and Abraham Kaplan, *Power and Society: A Framework for Political Inquiry* (New Haven, 1950), 67–68; William J. Goode, "Homan's and Merton's Structural Approach," in Blau, *Approaches to the Study of Social Structure*, 66–75; and Tom Bottomore, "Structure and History," *ibid.*, 159–71.

² The theme has been neglected not by American historians alone. In Heinz Eulau's phrase, if "the relationship between class and party has been one of the grand problems from Aristotle to Harold J. Laski, it has also been one of the most neglected areas of systematic theory and empirical analysis"; Eulau, *Class and Party in the Eisenhower Years* (Glencoe, Ill., 1962), 4.

³ Main, *The Social Structure of Revolutionary America* (Princeton, 1965), vii.

class structure betrays a preconceived notion on the part of those who perform it about the nature of that reciprocity. What interest in this issue actually betokens is a sensible insistence on enriching our understanding of politics.

The relatively few American historians influenced by the socialist tradition have, of course, engaged the theme. I find its treatment by some Marxist scholars unsatisfactory and do so for reasons that have nothing to do with our dissimilar ideological persuasions. Their discussion, as I follow it, relies heavily on a priori assumptions about the fundamentality, if I may coin a phrase, of the social and economic order in the relationship between class and politics, while relying hardly at all on the complex details that bear on the diverse and changing American social order or on the nation's equally diverse and changing politics.⁴ In his *An Economic Interpretation of the Constitution of the United States*, Charles A. Beard said that he was "required to discover at the very outset . . . what classes existed in the United States just prior to the adoption of the Constitution." Beard's performance did not fulfill his promise. After noting that a "lifetime of work" was supposedly necessary to unearth the requisite data, Beard settled for a hasty, impressionistic glance that he conceded was merely "an indication of the way in which the superficial aspects of the subject may be treated." His "social structure" is nothing more than two fragmentary pages labeling elements of the "working class" and slightly longer descriptions of different types of real and personal property holders.⁵ Admirers of Beard may find solace in the fact that classic discussions of the interaction between class and politics, from the works of Aristotle to those of Marx and Tocqueville, also base their interpretations on the sketchiest versions of the social structure.⁶

Jackson Turner Main's *The Social Structure of Revolutionary America* (1965) stands in solitary splendor as the only book-length study published by an American historian of the social structure of any period in American history.⁷ Sociologists have done a great deal of work that illuminates the American social structure, but their research focuses almost entirely on the twentieth century.⁸ A growing number

⁴ As I shall argue below, I find the Marxists' two-level class structure undescriptive of the actual social gradations that have usually divided American families, and I differ with them too in my perception of "class consciousness" and the light it throws on class. For an interesting recent discussion, see Reinhard Bendix, "Inequality and Social Structure: A Comparison of Marx and Weber," *American Sociological Review* [hereafter, *ASR*], 39 (1974): 149–61.

⁵ Charles A. Beard, *An Economic Interpretation of the Constitution of the United States* (New York, 1913), esp. chap. 2: "A Survey of Economic Interests in 1787." For a thoughtful and subtle recent re-evaluation of Beard's argument, see John Patrick Diggins, "Power and Authority in American History: The Case of Charles A. Beard and His Critics," *AHR*, 86 (1981): 701–30.

⁶ See Aristotle's *Politics*, trans. B. Jowett (Oxford, 1931); John Locke, *Two Treatises of Government*, ed. Peter Laslett (2d edn., Cambridge, 1967); Karl Marx and Friedrich Engels, *The Communist Manifesto*, trans. S. Moore and F. Engels (New York, 1948), 11, 31; and Alexis de Tocqueville, *Democracy in America*, 2 pts. (1835–40; Phillips Bradley edn., New York, 1954), 1: 3.

⁷ Not yet published is Tom W. Smith, "The Dawn of the Urban-Industrial Age: The Social Structure of Philadelphia, 1790–1830" (Ph.D. dissertation, University of Chicago, 1980). Appearing too late for consideration is Michael B. Katz et al., *The Social Organization of Early Industrial Capitalism* (Cambridge, Mass., 1982).

⁸ An exception to this rule is the set of three books by E. Digby Baltzell, which range over all of American history in their discussion of northeastern urban elites; see his *Philadelphia Gentlemen: The Making of a National Upper Class* (Glencoe, Ill., 1958), *The Protestant Establishment: Aristocracy and Caste in America* (London, 1965), and *Puritan Boston and Quaker Philadelphia: Two Protestant Ethics and the Spirit of Class Authority and Leadership* (New York, 1977). For a sampling of the massive sociological literature, see W. Lloyd Warner, *The Status System of a Modern Community* (New Haven, 1947); Joseph A. Kahl, *The American Class Structure* (New York, 1957); Bernard Barber, *Social Stratification: A Comparative Analysis of Structure and Process* (New York, 1957); W. Lloyd Warner

of historians have recently been researching the American social structure of earlier centuries and in a few instances have even attempted to relate social-structural to political developments—if only for particular communities at particular moments of the past.⁹ Emboldened by this recent work, as well as by the expanding political science and political-sociological discussions of the distribution of power in American communities, I shall offer some observations on the interrelationship between the class structure and politics over the whole course of American history.¹⁰ In the discussion that follows, I shall examine first the evolving social structure and then its interaction with politics.

ONE OF THE STRIKING FEATURES OF CLASS in the United States is the widespread American belief that classes do not exist here. Americans, as Woodrow Wilson observed, like to think that “this is the country where there is . . . no distinction of class, . . . no distinction of social status.”¹¹ One explanation of American historians’ neglect of class may be the affinity between scholarly and popular thinking, at

with Marchia Meeker and Kenneth Eells, *Social Class in America: A Manual of Procedure for the Measurement of Social Status* (New York, 1960); Leonard Riessman, *Class in American Society* (New York, 1959); Harold Hodges, *Social Stratification: Class in America* (Cambridge, Mass., 1969); Reinhard Bendix and Seymour Martin Lipset, eds., *Class, Status, and Power: Social Stratification in Comparative Perspective* (New York, 1966); Robert K. Merton, *Social Theory and Social Structure* (rev. edn., Glencoe, Ill., 1957); Anthony Giddens, *The Class Structure of the Advanced Societies* (New York, 1973); William M. Dobriner, *Class in Suburbia* (Engelwood Cliffs, N.J., 1963); Gerhard Lenski, *Power and Privilege: A Theory of Social Stratification* (New York, 1966); Maurice Zeitlin, *Classes, Class Conflict, and the State: Empirical Studies in Class Analysis* (Cambridge, Mass., 1980); and Jonathan H. Turner and Charles E. Starnes, *Inequality: Privilege and Poverty in America* (Pacific Palisades, Calif., 1976).

⁹ A small sample of this burgeoning historical literature includes—in addition to Main’s *The Social Structure of Revolutionary America*—Smith, “The Dawn of the Urban-Industrial Age: The Social Structure of Philadelphia, 1790–1830”; James Henretta, “Economic Development and Social Structure in Colonial Boston,” *William and Mary Quarterly*, 22 (1965): 79–92; Kenneth A. Lockridge, *A New England Town—The First Hundred Years: Dedham, Massachusetts, 1636–1736* (New York, 1970); Philip J. Grevett, Jr., *Four Generations: Population, Land, and Family in Colonial Andover, Massachusetts* (Ithaca, N.Y., 1970); Russell R. Menard, “From Servant to Freeholder: Status Mobility and Property Accumulation in Seventeenth-Century Maryland,” *William and Mary Quarterly*, 30 (1973): 37–64; Alan Kulikoff, “The Progress of Inequality in Revolutionary Boston,” *ibid.*, 28 (1971): 375–412; Richard J. Morris, “Wealth Distribution in Salem, Massachusetts, 1759–1799: The Impact of the Revolution and Independence,” *Essex Institute Historical Collections*, 114 (1978): 87–102; Edward Pessen, *Riches, Class, and Power before the Civil War* (Lexington, Mass., 1973); Frank Huffman, Jr., “Town and Country in the South, 1850–1880: A Comparison of Urban and Rural Social Structure,” *South Atlantic Quarterly*, 76 (1977): 366–81; D. L. A. Hackett, “The Social Structure of Jacksonian Louisiana,” *Louisiana Studies*, 12 (1973): 324–53; Lee Soltow, *Men and Wealth in the United States, 1850–1870* (New Haven, 1975); Richard K. Jones, “Stonington Borough: A Connecticut Seaport in the Nineteenth Century” (Ph.D. dissertation, City University of New York, 1976); Stuart Blumin, *The Urban Threshold: Growth and Change in a Nineteenth-Century American Community* (Chicago, 1976); Robert Doherty, *Society and Power: Five New England Towns, 1800–1860* (Amherst, Mass., 1977); Peter R. Knights, *The Plain People of Boston, 1830–1860: A Study in City Growth* (New York, 1971); Stephan Thernstrom, *Poverty and Progress: Social Mobility in a Nineteenth-Century City* (Cambridge, Mass., 1964); Merle Curti et al., *The Making of an American Community: A Case Study of Democracy in a Frontier Community* (Stanford, 1959); Alan Dawley, *Class and Community: The Industrial Revolution in Lynn* (Cambridge, Mass., 1976); Kathleen Neils Conzen, *Immigrant Milwaukee, 1836–1860: Accommodation and Community in a Frontier City* (Cambridge, Mass., 1976); Michael P. Weber, *Social Change in an Industrial Town: Patterns of Progress in Warren, Pennsylvania, from Civil War to World War I* (University Park, Pa., 1976); John N. Ingham, *The Iron Barons: A Social Analysis of an American Urban Elite, 1874–1921* (Westport, Conn., 1978); and Carl V. Harris, *Political Power in Birmingham, 1871–1921* (Knoxville, Tenn., 1977).

¹⁰ In view of the scope and complexity of the theme and the obvious impossibility of doing full justice to it in a journal essay, I shall paint in broad strokes and focus on a select few aspects of the topic.

¹¹ Wilson, *The New Freedom: A Call for Emancipation of the General Energies of a People* (1912; reprint edn., Engelwood Cliffs, N.J., 1961), 24–25.

least with regard to this subject. To judge from public response to sociological questionnaires and other evidence, American attitudes have not changed very much in the two generations since Wilson made his remark. The public, in the phrase of a modern student, continues to display "a considerable amount of either ignorance or vague knowledge about the system of stratification" or class hierarchy "in the contemporary United States."¹² That, in the popular mind, classes do not exist in America or that, if they do, almost everyone belongs to the great middle class are notions that appear to confirm the validity of the sociological dictum that "what people think about society does not necessarily correspond to social reality." Yet, popular perceptions of reality, no matter how dubious their accuracy, themselves become a part of reality since, as an equally compelling sociological dictum holds, "if men define situations as real, they are real in their consequences."¹³ One consequence of how Americans "define their social situation" has been their indifference to a great quantity of evidence demonstrating that distinct classes have existed throughout American history.

The United States is no exception to Heinz Eulau's observation that "no society is conceivable without some sort of class structure."¹⁴ True, at no time in its national history has the United States had a formal class structure in which individuals and families were assigned differential places having the force of custom or law. Here, as elsewhere, however, people have been divided, if informally, into social groups, clusters, or classes that differ markedly from one another in significant respects, measurable and immeasurable, tangible and intangible. As I shall try to show, few if any things have touched or influenced the lives of most Americans as vitally as the positions or levels they have occupied in their society's class structure. But, before proceeding, let me first make clear what I mean by class.

Given the abstractness and complexity of class, the multidimensionality of its component elements, the unavoidable imprecision attending evaluation of its intangible ingredients (such as status and prestige), and the subjectivity involved in ranking such of its objective features as occupation and residence, controversy over the meaning of the term is inevitable. The number of definitions of class comes close to matching the number of scholars offering these definitions.¹⁵ Who speaks of class, therefore, must perforce present his own definition of the term. I regard class as determined by a combination or blending of wealth, the means and occupations by which it has been accumulated, the prestige, quality, and relative irksomeness of these means, the style of living (including the uses of leisure) made possible by one's or one's family's wealth and income, the social reputes the family enjoys (which rests on, among other things, its racial and ethnic identity, its religious

¹² Barber, *Social Stratification*, 197, 209.

¹³ W. I. Thomas [at the time, the "dean of American sociologists"], as quoted in Merton, *Social Theory and Social Structure*, 421.

¹⁴ Eulau, *Class and Party in the Eisenhower Years*, 15.

¹⁵ In addition to the literature cited in note 8, above, see Erik Olin Wright and Luca Perrone, "Marxist Class Categories and Income Inequality," *ASR*, 42 (1977): 32-55; Howard Aldrich and Jane Weiss, "Differentiation within the United States Capitalist Class: Workforce Size and Income Differences," *ibid.*, 46 (1981): 279-90; Edward Pessen, "The Social Configuration of the Antebellum City: An Historical and Theoretical Inquiry," *Journal of Urban History*, 2 (1976): 267-306; and Michael B. Katz, "Social Class in North American Urban History," *Journal of Interdisciplinary History* [hereafter, *JIH*], 11 (1981): 579-605.

affiliation, the schooling it provides its young, the organizations to which its members belong and the role they play in these organizations, the standing of the social circle it travels in and of the families with which it is intimate, and how long it has held such standing), and the influence and power the family commands in its own and in the larger community. I offer this definition not because, in the language of the day, it makes the concept “operational”—that is, subject to quantitative or statistical analysis and graphic or tabular representation—but, rather, because I think it does justice to the meaning of the term class as it has long been used and implicitly understood by scholars and informed laity alike.

I am not saying that mine is the correct definition. I am saying only that I believe it is a sensible definition. Definitions of class that stress one or another single “indicator” and the Marxist view that a class is a group, such as workers or capitalists, whose members share a distinctive relationship to the means of production are no doubt easier to work with than is my more eclectic version of class. I continue to cling to the notion, however, that the chief virtue a scholarly definition of a concept can aspire to is to approximate as closely as possible the actual complexity of the phenomenon it seeks to define. My definition is, of course, no less imperfect, no less beset with problems than are others. Discussion of some of the problems may be edifying.

In assigning individuals and families to a given class level on the basis of a blending of diverse characteristics and attributes, we appear to assume that their wealth, income, lifestyles, prestige, influence, and power are essentially commensurate, each with all of the others. This assumption is not invariably valid. Many individuals can rate “high” in one or two of these categories and rank “low” in others. Prince Charles’s recent observation that, however much he may influence the thinking and actions of many people, he has no real power seems apt. Many who have amassed much wealth enjoy little prestige because of the less-than-admired means by which they have amassed it. In contrast, a brilliant and respected minister of little wealth such as Woodrow Wilson’s father could be accorded the highest standing in his Virginia and Georgia communities. In assigning class position to an individual whose rankings for each of the components of class are dissimilar, the scholar must make a subjective judgment that is based on the particulars of each case. Weighting these elements of class according to a careful estimate of their comparative value seems a sensible procedure. It is useful to remember, however, that these comparative weights may have to be modified, depending on the time, place, and context. No perfect calculus of class ranking seems possible.

For most of American history, I believe that wealth has been the single best clue to class position. (For that matter, it evidently was in the late Tudor and early Stuart England described by Lawrence Stone; families whose wealth dipped below the level deemed appropriate were dropped from the ranks of the aristocracy.) Main, who distinguished between economic class and social class, has concluded that in colonial and Revolutionary America “the acquisition of wealth opened all doors” to the “socially ambitious.” In the antebellum decades when, to paraphrase Juvenal, wealth smelled sweet no matter how earned, nothing seemed a better clue to class

standing, whether in the cotton South or the commercial North. That gangsters, athletes, entertainers, popular writers, and others, whose means of earning income are not universally regarded as socially honorific, have nevertheless amassed varying fortunes has in our own time clouded the once-clear symbol of wealth as a clue to class. Fortunes made in industry, commerce, finance and investment, land, and the law, and inheritance of wealth thus accumulated, are probably more respected because they are likely to be older, more stable, more socially prestigious, and often larger than those made through other means.¹⁶

A small problem that arises is of particular interest to those who stress occupational prestige in defining class. If we accept the view, as many sociologists do, that the family rather than the individual is the fundamental atomic unit of class, what is one to make of two young adults, members both of an extremely wealthy and admired family, who have occupations of dissimilar prestige levels? Shall they therefore be assigned to different classes? Clearly, occupation must in some instances be subordinated to other indicators of class. And the internal differentiation within occupations, a differentiation that divides storefront merchants from great overseas operators and lawyers who scratch out a living servicing the poor from solicitors to the corporate mighty, requires us to probe for evidence that goes beyond occupational levels.¹⁷

Some scholars, Marxists included, have attributed great significance to consciousness as an index of class. I am inclined to be skeptical. That individuals may be "class conscious," even militantly or stridently so, tells us more about the strength of their ideological convictions than about their position in the social structure. Consciousness, as no less non-Marxist authorities than Paul F. Lazarsfeld and Robert K. Merton once told us, may be, as a result of indoctrination, false consciousness.¹⁸ It is precisely because consciousness may, for whatever reasons, be unrevealing of people's actual situation that I am inclined to discount its usefulness as a clue to the class affiliation of individuals. Social consciousness is not only impossible to ascertain precisely but, when ascertained, may be more indicative of their status goals than of their actual station. Ideas have great influence on people, powerfully affecting how they perceive the fairness of their society's social order, its degree of equity, and the comparative opportunities it affords. But that is quite another matter from accepting at face value people's ideas about their own class position.

¹⁶ Stone, *The Crisis of the Aristocracy, 1558–1641* (London, 1965), 52–53, 56–58; Main, *The Social Structure of Revolutionary America*, 219; Pessen, "Social Configuration of the Antebellum City," 278–80; Gavin Wright, *The Political Economy of the Cotton South: Households, Markets, and Wealth in the Nineteenth Century* (New York, 1978); and Barber, *Social Stratification*, 42–43, 375–87.

¹⁷ Knowledgeable discussions of the strengths and weaknesses of occupation as an index to social class include Peter M. Blau and Otis Dudley Duncan, *The American Occupational Structure* (New York, 1967); Paul Hatt, "Occupation and Social Stratification," *American Journal of Sociology*, 55 (1950): 533–47; Robert W. Hodge et al., "Occupational Prestige in the United States, 1925–1963," *ibid.*, 70 (1964): 287–88; Edward Pessen, "The Occupations of the Antebellum Rich: A Misleading Clue to the Sources and Extent of Their Wealth," *Historical Methods Newsletter*, 5 (1972): 49–52; and Michael B. Katz, "Occupational Classification in History," *JIH*, 3 (1972): 63–88. For the varied social significance of being a "mechanic" in the early nineteenth century, see Howard B. Rock, *Artisans of the New Republic: The Tradesmen of New York City in the Age of Jefferson* (New York, 1979).

¹⁸ Lazarsfeld and Merton, "Mass Communication, Popular Taste, and Organized Social Action," in B. Rosenberg and D. M. White, eds., *Mass Culture: The Popular Arts in America* (1957), as quoted in R. Miliband, *The State in Capitalistic Society* (London, 1969), 220.

The classic vertical rendering of the class structure, which divides the social hierarchy into an upper, middle, and lower class, with each in turn, at minimum, subdivided into an upper and lower group, leaves the impression that each class between the very highest and the lowest is not only adjacent to but also equidistant from the class just above and just below it. This is not, however, the case. The upper-middle class is much closer in most respects to the lower-upper than to the lower-middle class. For, as Richard Parker has observed, "being middle class can mean comfort bordering on opulence; but it can also mean outright poverty or deprivation that is only one step removed from poverty."¹⁹ Nor is it just a matter of material circumstances. The prestige and the community roles of upper-middle-class professionals or business people (like Gerald R. Ford's father) are also more akin to the standing and behavior of the class just above than they are to the class just below.

If the gulf between the classes is to be measured sensibly, it must not be measured altogether quantitatively. Franklin D. Roosevelt's father left an estate of less than one-half million dollars. This sum is much closer to zero than it is to the value of the fabled estates amassed by the great late nineteenth-century wealthholders. Insofar as wealth is an index to class position, a mechanical appraisal of the Roosevelt fortune would suggest that the family was therefore much closer to the lowest than to the highest class. But such a judgment would be nonsensical. The point is not simply that FDR's family rated high by indices other than wealth. It is rather that, once a certain standard of wealth—and in dollar terms, it need not be an awesome figure—enables its possessors not only to be spared what Tocqueville called the "sting of want" but also to enjoy a materially resplendent life and the absence of any need to work for a living, it then matters little that the sum in question is arithmetically closer to zero than it is to the sums owned by the richest of the rich. To put the point in mathematical terms, that a sum of \$500,000.00 may be only 1 percent of the distance from destitution (\$0.00) to opulence (\$50,000,000.00) does not mean that more than a fraction of 1 percent of the population possess it. Such excellent biographers of the Republican Roosevelt as Henry F. Pringle, early in the century, and William Henry Harbaugh, more recently, could refer to TR's father as an upper-middle-class man of modest wealth, despite his ability to confer on his son a quality of life and a social standing that were beyond the dreams of more than 99 percent of the population. The problem appears to lie in part in ranking a family's wealth level by an unrealistically absolute arithmetical standard and in part in the misleading implication of the term middle class.²⁰

I have spoken, and I shall continue to speak, of the American social or class structure as though it were a single, uniform entity. For, in trying to understand the nature of the ties between class and politics, it is useful to think of a single or average class structure against which political events can be appraised. But the term "the American class structure" is really a mere figure of speech. There is not now, nor has there been at any earlier time, a single American class structure. At every

¹⁹ Parker, *The Myth of the Middle Class: Notes on Affluence and Equality* (New York, 1972), 6–7.

²⁰ Pringle, *Theodore Roosevelt: A Biography* (New York, 1931); and Harbaugh, *Power and Responsibility: The Life and Times of Theodore Roosevelt* (New York, 1961).

moment in its history, the nation has been composed of communities of diverse sorts, each of which has a distinctive class structure. The nature of social classes and the proportions enrolled in them, which determines the shapes they assume, vary according to the age, size, population, location, natural resources, wealth, economic practices, and complexity of the community. And with the passage of time the class system of a community is likely to be modified, whether by new patterns of immigration, technological innovation, or other forms of historical change.²¹ The search for a unitary American class structure is, therefore, an exercise in imagination. A comprehensive model that approximates or averages out the class structures of all communities is precisely like no actual community. This is not to say that such a model is without value. The classic "textbook manor" is like no actual rural community that ever existed, yet teachers of medieval economic history have put the concept to good use. If used informedly and sensibly, the "textbook American social structure" can also be useful, particularly in trying to evaluate the interrelationship between other facets of American life and this (diverse and evolving) class structure.

AMERICA'S ALWAYS DISSIMILAR AND CHANGING CLASS STRUCTURES do have significant features in common, similarities that imbue the whole with a degree of homogeneity. The most obvious commonality is a stratification that divides the population into five or six significantly dissimilar classes. W. Lloyd Warner and his disciples divide the highest class into an upper-upper and a lower-upper level only when a community is old enough to contain a number of patrician or "old families," whose eminence and prestige outshine those who are merely wealthy, successful, and powerful. Since I agree with those scholars who believe that the distinction between upper-upper and lower-upper is not a matter of family antiquity alone, I believe there is much to be said for a six-level class structure.

The particular ingredients of membership in a given class have, of course, changed over time. Where upper-upper-class Southerners in Thomas Jefferson's or John C. Calhoun's time might each own several hundred slaves and tens of thousands of acres of valuable fertile land, their modern counterparts are likely rather to own oil wells, factories, transportation franchises, newspaper chains. Community roles, such as justice of the peace, that once carried great political influence and prestige have today diminished drastically in power and standing. A Western lower class once composed largely of poorly paid, overworked farm laborers more recently consists of men of marginal industrial skills and erratic employment.²² Higher education that only two generations ago was accessible to

²¹ For examples of the changes overtaking the class structure of different communities during the course of a single generation, see Dawley, *Class and Community*; Michael B. Katz, *The People of Hamilton, Canada West: Family and Class in a Mid-Nineteenth-Century City* (Cambridge, Mass., 1975); Conzen, *Immigrant Milwaukee*; Edward Pessen, "A Social and Economic Portrait of Jacksonian Brooklyn," *New-York Historical Society Quarterly*, 55 (1971): 318–53; Weber, *Social Change in an Industrial Town*; and Mary P. Ryan, *Cradle of the Middle Class: The Family in Oneida County, New York, 1790–1865* (Cambridge, Mass., 1981).

²² David E. Schob, *Hired Hands and Plowboys: Farm Labor in the Midwest, 1815–1860* (Urbana, Ill., 1975); and Donald L. Winters, "Tenancy as an Economic Institution: The Growth and Distribution of Agricultural Tenancy in Iowa, 1850–1900," *Journal of Economic History*, 37 (1977): 382–408.



Figure 1: "Reading the Tribune" (1850), oil by William Sidney Mount (1805–68).
Reproduced courtesy of the Suffolk Museum and Carriage House, Stony Brook, New York.

less than 2 percent of the eligible young adult population has today become a commonplace, attainable by about 40 percent; it has obviously lost, thereby, its onetime significance as a clue to social position. These inevitable changes in the ingredients or in the weight to be ascribed the ingredients of membership in this or that class have resulted in the disappearance neither of a stratified class structure nor of the wide gaps that separate the higher from the lower levels.

Let me try succinctly to depict the important distinguishing characteristics of each of the classes. At any time in American history the upper-upper classes possess wealth that enables them to live lives of relative material splendor. They do so not by toilsome and irksome labor but by occupations that require mental effort, in many cases only the mental effort needed to see to the hiring and supervision of those who, in turn, supervise the labor of others, by investment, and by inheritance. Great landholdings or large-scale overseas commerce or ownership of large plants or corporate directorships or highly successful legal or medical practice, in addition

to successful investment and fortunate inheritance, are the means through which they accumulate and add to their wealth. Their fortunes are substantial enough to permit adult family members to devote themselves entirely to nonutilitarian pursuits and diversions. Their wealth, the admired means by which it is earned, and their dazzling and often well-publicized lifestyle appear to confer on the upper classes as great a monopoly over social eminence as they enjoy over material resources. Influence is manifested in diverse ways. Many studies of the socially purposive voluntary associations that since before Tocqueville's time have complemented and at times surpassed the power of government reveal that their leaderships typically have been drawn from the social upper crusts.²³ The chief distinction between upper-upper- and lower-upper-class families appears to depend not only on how long they have enjoyed their wealth, renown, and influence but on subtle gradations of social acceptability. The Kennedys of Brookline, for example, were not part of the most prestigious Boston set even after they attained the requisite wealth and public standing. American novels and biographies report many instances of relatively nouveau riche members of the lower-upper class trying desperately and not always successfully to crack the charmed circle inhabited by the upper-upper class, as the latter dine, party, vacation, attend elite social clubs, and intermarry exclusively among their own sort.

The upper-middle class, as I have noted, live extremely well, if not opulently, and do so through socially honorific occupations, whether in land, small business, the professions, or the arts. They play an active, if not always a dominating, part in influential community organizations. Members of the upper-middle class are typically admired by those below, who, according to a number of opinion surveys, see few if any differences between the upper-middle class and the classes above them.²⁴ The lower-middle class is closer, qualitatively, to the upper-lower than to the upper-middle class. Small shopkeepers, clerks, and low-level white-collar workers, foremen and skilled industrial workers in urban communities, small farmers of modest property in rural areas, they appear neither to suffer want nor to

²³ William Cutler III, "Status, Values, and the Education of the Poor: The Trustees of the New York Public School Society, 1805–1853," *American Quarterly*, 24 (1972): 69–85; Raymond A. Mohl, *Poverty in New York, 1783–1825* (New York, 1971); Roy Lubove, "The New York Association for Improving the Condition of the Poor: The Formative Years," *New-York Historical Society Quarterly*, 43 (1959): 307–27; Mary Bartlett Cowdrey, *The American Academy of Fine Arts and American Art-Union* (New York, 1953); M. J. Heale, "From City Fathers to Social Critics: Humanitarianism and Government in New York City, 1790–1860," *Journal of American History*, 63 (1976): 21–46; Pessen, *Riches, Class, and Power before the Civil War*, chap. 12; Walter S. Glazer, "Participation and Power: Voluntary Associations and the Functional Organization of Cincinnati in 1840," *Historical Methods Newsletter*, 5 (1972): 151–68; Richard D. Brown, "The Emergence of Voluntary Associations, Massachusetts, 1760–1830," *Journal of Voluntary Action Research*, 2 (1973): 64–73; Clifford S. Griffin, *Their Brothers' Keepers: Moral Stewardship in the United States, 1800–1865* (New Brunswick, N.J., 1960); Benjamin J. Klebaner, "The Home Relief Controversy in Philadelphia, 1782–1861," *Pennsylvania Magazine of History and Biography*, 78 (1954): 412–23; Roy Lubove, *The Professional Altruist: The Emergence of Social Work as a Career, 1800–1930* (Cambridge, Mass., 1965); Ronald Story, "Class and Culture in Boston: The Athenaeum, 1807–1860," *American Quarterly*, 27 (1975): 178–99; David J. Rothman, *The Discovery of the Asylum: Social Order and Disorder in the New Republic* (Boston, 1971); Charles R. Wright and Herbert H. Hyman, "Voluntary Association Memberships of American Adults: Evidence from National Sample Surveys," *ASR*, 22 (1958): 286–94; David L. Sills, *The Volunteers: Means and Ends in a National Organization* (Glencoe, Ill., 1957); and Murray Hausknecht, *The Joiners* (New York, 1962).

²⁴ It is a sociological commonplace, demonstrated by many opinion surveys, that the lower people's places on the social ladder, the more prone they are to lump together—in a single undifferentiated mass—persons and families that stand near the top.

enjoy great material comfort. Lower-middle-class families usually have slight community involvement or influence over public affairs.

The lower classes are almost totally lacking in property, prestige, and power. Urban breadwinners in the upper-lower echelon are usually semiskilled; in rural milieus they are likely to have been marginal farmers whose small farms were heavily mortgaged. If they are unable to afford attractive possessions, amenities, and uses of leisure, let alone luxuries, they are nevertheless likely to possess and have access to the material necessities. The lower-lower class includes, among its more unfortunate members, those who are the American equivalent of a "lumpen-proletariat." This class contains tenant farmers and agricultural laborers, the unskilled, almost all pre-Civil War blacks, the chronically unemployed, and the unemployable, has little or no margin to fall back on in hard times, and—even in the era of the welfare state—lives grim lives that fall below what public agencies describe as minimally acceptable.

The great question concerns the proportions of the population enrolled in each of the classes over the passage of time. Precision is, of course, not at all possible in trying to answer such a question, in view not only of the indeterminacy surrounding the definition of class but the obvious impossibility of assigning exact numbers of families to each class for all periods in American history. The best we can hope for are sensible estimates that are based on appropriate evidence. Several admired sociologists have suggested that for the mid-twentieth century about 1 percent of the community should be placed in the upper-upper class, 2 percent in the lower-upper, 9 to 10 percent in the upper-middle, 28 to 36 percent in the lower-middle, 33 to 35 percent in the upper-lower, and 17 to 25 percent in the lower-lower class.²⁵

As the scholars presenting them would doubtless have agreed, these are something less than figures carved in stone; alter the weight affixed to any of the criteria defining class and the percentage of families located in one or another of the classes also changes. And yet these do appear to be sensible estimates that usefully represent the shape of the sharply stratified American class structure. And there appears to be little in the historical record to suggest that the pattern for distributing American families across the social ranks was earlier less skewed toward the lower levels, less imbalanced from top to bottom, than it is in this century.

The proportion of families that appear to have owned little or no wealth in the late eighteenth and the nineteenth century equaled and in many instances surpassed the proportions more recently identified with the two lower classes.²⁶

²⁵ Warner and Lunt, *The Status System of a Modern Community*, 203; August B. Hollingshead, *Elmstown's Youth* (New York, 1949), 37–40; Kahl, *American Class Structure*, 187; and Carson McQuire, "Social Stratification and Mobility Patterns," *ASR*, 15 (1950): 200.

²⁶ Studies of the distribution of wealth include Henretta, "Economic Development and Social Structure in Colonial Boston"; Kulikoff, "The Progress of Inequality in Revolutionary Boston"; James T. Lemon and Gary B. Nash, "The Distribution of Wealth in Eighteenth-Century America: A Century of Changes in Chester County, Pennsylvania, 1693–1802," *Journal of Social History* [hereafter, *JSocH*], 2 (1968): 1–24; Morris, "Wealth Distribution in Salem, Massachusetts"; Alice Hansen Jones, *Wealth of a Nation: The American Colonies on the Eve of the Revolution* (New York, 1980); Pessen, *Riches, Class, and Power before the Civil War*; Randolph B. Campbell and Richard G. Lowe, *Wealth and Power in Antebellum Texas* (College Station, Texas, 1977); Doherty, *Society and Power*; Wright, *The Political Economy of the Cotton South*; Robert E. Gallman, "Trends in the Size Distribution of

The tax and census data on which most studies of wealth distribution have relied are themselves imperfect. But, as those of us who have worked with this evidence have come to know, in underestimating above all the fraction of their communities' wealth owned by small numbers of the great wealthholders, it underestimates too the actual maldistributions of wealth that have usually obtained or, in more modish terms, the Gini coefficients for measuring inequality.²⁷ A variety of other data also testifies to the presence of sharp social stratification in the United States.

Fairly good evidence available on the agricultural population since the mid-nineteenth century reveals that between 1850, when farm families constituted two-thirds of the population, and 1900, when they had diminished to one-third of the whole, farms were small, tenancy was surprisingly common, and the wages of farm labor averaged less than \$15 per month. Between 1900 and the middle of the twentieth century, as the farm population was more than halved, the wages of farm labor reached a monthly average of \$25, while independent farmers netted on the average less than \$1,000 per year (a figure that would be significantly lower if the earnings of the small number of inordinately rich and successful operators were not included). Good data on industrial labor in the nineteenth century discloses a surprisingly narrow gap between the wages of laborers and skilled craftsmen. In the late nineteenth century, skilled workers were making less than \$500 per year, unskilled workers slightly more than half that sum. That Dwight David Eisenhower's father, who is treated by most of the president's biographers as an economic failure, was at the time earning about five times as much as the average American worker, in his modest position in his brother-in-law's creamery, suggests the unimpressiveness of the average working-man's income. In the twentieth century, labor, industrial and white collar, has constituted about two-thirds of the work force.

In contrast, the professions of law, the ministry, and medicine, like bank management and corporate officialdom (occupations, all, of the not quite 15 percent of heads of families assigned by sociologists to the three upper ranks—the upper-upper, the lower-upper, and the upper-middle classes), have each contained no more than one-third of 1 percent or, when combined, less than 2 percent of adult income earners in the population. At mid-twentieth century, businessmen and professionals were in the highest echelons, within the top 1 percent of income

Wealth in the Nineteenth Century: Some Speculations," in Lee Soltow, ed., *Six Papers on the Size Distribution of Wealth and Income* (New York, 1969), 1–25; Craig Buettinger, "Economic Inequality in Early Chicago, 1840–1850," *JSocH*, 11 (1978): 413–18; G. K. Holmes, "The Concentration of Wealth," *Political Science Quarterly*, 8 (1893): 589–600; C. B. Spahr, *The Present Distribution of Wealth in the United States* (New York, 1896); Herman P. Miller, *Rich Man, Poor Man* (New York, 1971); Gabriel Kolko, *Wealth and Power in America: An Analysis of Social Class and Income Distribution* (New York, 1965); Robert J. Lampman, *Changes in the Share of Wealth Held by Top Wealthholders, 1922–1953* (New York, 1960), and *The Share of Top Wealth-Holders in National Wealth, 1922–1956* (Princeton, 1962); and James D. Smith and Stephen D. Franklin, "The Concentration of Personal Wealth, 1922–1969," *American Economic Review* [hereafter, *AER*], 64 (1974): 162–67. For brief overviews, see T. E. Vadney, "Wealth and Income in American History: The Persistence of Inequality," *Canadian Review of American Studies*, 8 (1977): 89–94; and Smith, "The Dawn of the Urban-Industrial Age."

²⁷ For informed discussions of the strengths and weaknesses of these data, see Soltow, *Men of Wealth in the United States*; and Buettinger, "Economic Inequality in Early Chicago." I have discussed the problem in *Riches, Class, and Power before the Civil War*, 11–22.

earners. Those less than dramatically successful lawyers, doctors, and business people, whose income is often described as "modest" and whose class affiliation is "middle" or "upper middle," in fact did better than 99 percent of their adult contemporaries. In view of the actual proportions of eligible young people attending college before 1900, those who assume that such attendance was confined almost exclusively to members of the upper-middle and upper classes unwittingly reduce the representation in the three highest classes to about 2 percent of the population.²⁸ The class structure was skewed, no doubt, but not quite as skewed as the evidence on the proportions of business people, professionals, and college students in the eligible populations suggests.

A great deal of work remains to be done to refine our knowledge and understanding of important changes that have occurred in the shape and proportions of families identified with the different classes over the course of American history. The reigning chronological periodizations that American historians rely on to divide up and to introduce significant transitions in American national history are not likely to be very useful to scholars investigating the chief historical influences on class structure, given the traditional emphasis on great political figures and single dramatic themes that are characteristic of these periodizations. Students searching for the sources of change in the American social structure are better advised to examine the consequences of more likely historical events: the American Revolution; the rise of banks and private corporations; drastic changes in immigration patterns, first after 1830 and then a half-century later; the end of slavery; the emergence of a mature industrialized economy; the continuing urban revolution and the accompanying population decline in rural America; the Sixteenth Amendment; antimonopoly and business regulation; recurring financial crises and economic depressions; changing attitudes toward racial and religious minorities; the boom in higher education; the continuing technological revolution of the twentieth century; the growth of leisure.²⁹ Even in the absence of the greater precision that all students of the American social structure seek, however, the not insubstantial evidence we have to date accumulated does throw the contours of that structure into fairly sharp relief.

The most salient features of the diverse and changing American class structure are the vast gulfs separating the top and bottom layers, the inordinate share of wealth, prestige, and influence possessed by the few in the upper clusters, the dramatically disproportionate population occupying the lower levels, and the striking degree of stability that appears to have attended the stratified social structure over time. Counterposed against the forces of historical change are forces that have operated to maintain and widen the gulf between classes and perpetuate the imbalance in the class structure. In a sense, all of these elements of stability have a political foundation, since they have been sustained by law as well as custom and, when they take the form of beliefs, are beliefs that have to a large extent been

²⁸ The single best repository for most of these data is U.S. Census Bureau, *Historical Statistics of the United States, Colonial Times to 1972* (2d edn., Washington, 1975).

²⁹ I have developed these points more fully in "Toward a Clearer Historical Overview of the Evolving American Class Structure," paper presented at the annual meeting of the Social Science History Association, held in Nashville, Tenn., October 23, 1981.

shaped by actors in the American political drama. The dimension of these factors, each of which is capable of inspiring a substantial literature, suggests the wisdom of doing no more in this essay than merely alluding to them as explanations of the continuing social stratification in America. The catalogue includes the bedrock institutions of the American capitalistic order: private ownership of property, wills and private inheritance, the wage system; the ideological conservatism of the American people; the operation of a variety of "social safety valves," having nothing in common with Turner's frontier, except insofar as they allay the danger of social explosions; nationalism and war; the near exorcism of social radicalism in the contemporary United States; and the performance of the major parties that have controlled the American political system.³⁰

It has rightly been argued that the disclosure of inequalities in American life is no proof of inequity. Experience indicates that perfect equality exists only in the minds of men and in the blueprints some of them draw for the good society—not in life. In view of the seeming inevitability of inequality in the distribution of wealth, status, influence, and power, what could be more fair than a society in which success and failure are either ephemeral states, the common experience of almost all men if they live long enough, or, when more enduring, simply reflections of men's differing abilities and character? Alexis de Tocqueville in the 1830s and many contemporary and later admirers of his *Democracy in America* discerned a remarkable "equality of condition" in American life. In conceding that some people do far better than others at any given moment, they explain that, in the kaleidoscopic American society, constant excitation and changes of condition are the norm, since young men who were poor yesterday become rich today and are likely to fall by tomorrow.³¹ A recent variant on this viewpoint argues that disparities in wealth ownership in the United States are due largely to the disparate earning power of people as they get older, prior to retirement: the relatively low incomes and slight wealth of younger people soar dramatically as they mature; "life-cycle models" suggest that inequality of material condition among Americans can be explained almost entirely by age differences.³²

Complementing the argument that wealth and poverty are passing phases in the average American's life cycle is the functionalist theory holding that, in the United States as elsewhere, social inequality is a "device by which societies insure that the most [attractive and] important positions are conscientiously filled by the most qualified persons"; those at the top are there because they deserve to be.³³ This is

³⁰ For a fuller discussion of the social implications and consequences of the other-than-political "forces of stability," see *ibid.* The effects of politics on the class structure are discussed below.

³¹ For discussion of Tocqueville's influence on scholars' conceptions of the American social structure, see Lynn L. Marshall and Seymour Drescher, "American Historians and Tocqueville's *Democracy*," *Journal of American History*, 55 (1968): 512–32; and Pessen, *Riches, Class, and Power before the Civil War*, pt. 1.

³² Morton Paglin, "The Measurement and Trend of Inequality: A Basic Revision," *AER*, 65 (1975): 598–609; Robert E. Gallman, "Professor Pessen on the 'Egalitarian Myth,'" *Social Science History* [hereafter, *SSH*], 2 (1978): 194–207, and "The 'Egalitarian Myth,' Once Again," *ibid.*, 5 (1981): 223–34.

³³ Kingsley Davis, *Human Society* (New York, 1949), 367; Kingsley Davis and Wilbert E. Moore, "Some Principles of Stratification," *ASR*, 10 (1945): 242–49; Talcott Parsons, "A Revised Analytical Approach to the Theory of Social Stratification," in Reinhard Bendix and Seymour M. Lipset, eds., *Class, Status, and Power* (Glencoe, Ill., 1953), 92–128; Suzanne Keller, *Beyond the Ruling Class: Strategic Elites in Modern Society* (New York, 1963); and Pitirim Sorokin, *Social Mobility* (New York, 1927).

hardly a new argument. In his famous *Federalist* essay, James Madison made the largely overlooked point that “the possession of differing degrees” of property ostensibly results from men’s “different and unequal *faculties* of acquiring property.” Embedded in Andrew Jackson’s almost equally famous bank veto message—that the overwrought Nicholas Biddle treated as though it were a call for the confiscation of private wealth—is the observation that social and economic inequality is the inevitable fruit of men’s unequal talents, industry, and virtue. In *The American Democrat*, James Fenimore Cooper spoke for the small minority who believed that inequality of condition was increasingly the rule in antebellum America. But Cooper attributed inequality in material circumstances to the operation in this country of an equality of opportunity, which supposedly rewards the intelligent and the industrious while punishing those who lack what it takes.³⁴ Space and time permitting, a lengthy catalogue could be assembled of statements by influential Americans asserting that it is primarily to the quality of their own efforts that men owe the success or failure they achieve in life. Horatio Alger was wise enough to attribute to luck much of the credit for worldly success. But, since luck is akin to a democratic lottery, equally available to all, and insofar as it is best exploited by the talented and the alert, its operation is hardly inequitable.

None of these arguments for the essential equity of the American class structure can withstand critical scrutiny. The portrait of a kaleidoscopic social scene in which successful men are self-made and poverty and failure ephemeral states rests largely on logic, undocumented assumptions, and impressionism—or whatever examples came to hand. Relying on masses of representative historical data, modern sociologists and social historians have disclosed that very few of the rich and successful during any period of American history ascended from the lower ranks and that movement up or down across the lower and middling ranks has been surprisingly modest and typically “small-distanced,” usually representing a change not in the social class but in some of the material circumstances of those who experience it.³⁵ For that matter, social mobility is not incompatible with the continuation of a sharply stratified society. The ascent or descent of persons into new social classes can occur, as Bernard and Elinor Gellert Barber have noted,

³⁴ Madison, *The Federalist*, no. 10 (italics mine); Jackson, Bank Veto Message, July 10, 1832, in James D. Richardson, ed., *Messages and Papers of the Presidents, 1789–1897*, 2 (Washington, 1899): 576–91; and Cooper, *The American Democrat* (New York, 1838), 73.

³⁵ For examples of this last point, see Thernstrom, *Poverty and Progress*; Stuart Blumin, “Mobility and Change in Ante-Bellum Philadelphia,” in Stephan Thernstrom and Richard Sennett, eds., *Nineteenth-Century Cities: Essays in the New Urban History* (New Haven, 1969), 165–206; Seymour Martin Lipset and Reinhard Bendix, *Social Mobility in Industrial Society* (Berkeley and Los Angeles, 1959); Edward Pessen, ed., *Three Centuries of Social Mobility in America* (Lexington, Mass., 1974); Clyde Griffen and Sally Griffen, *Natives and Newcomers: The Ordering of Opportunity in Mid-Nineteenth-Century Poughkeepsie* (Cambridge, Mass., 1977); Peter R. Decker, *Fortunes and Failure: White Collar Mobility in Nineteenth-Century San Francisco* (Cambridge, Mass., 1978); Michael P. Weber and Anthony E. Boardman, “Economic Growth and Occupational Mobility in Nineteenth-Century Urban America: A Reappraisal,” *JSoH*, 11 (1977): 52–74; Weber, *Social Change in an Industrial Town*; and Gordon W. Kirk, Jr., *The Promise of American Life: Social Mobility in a Nineteenth-Century Immigrant Community—Holland, Michigan, 1847–1894* (Philadelphia, 1978). For the social origins of the wealthy, see Main, *The Social Structure of Revolutionary America*; Pessen, *Riches, Class, and Power before the Civil War*; Mabel Newcomer, *The Big Business Executive: The Factors that Made Him, 1900–1950* (New York, 1955); Frances W. Gregory and Irene D. Neu, “The American Industrial Elite in the 1870s: Their Social Origins,” in William Miller, ed., *Men and Business: Essays in the Historical Role of the Entrepreneur* (Cambridge, Mass., 1952), 193–209; William Miller, “American Historians and the Industrial Elite,” *JEH*, 9 (1949): 184–208; Frank W. Taussig and C. S. Joslyn, *American Business Leaders: A Study in Social Origins and Stratification* (New York, 1932); “The Nine Hundred,”

"without transforming the stratification system as a whole," particularly when, in the phrase of Keith Hope, "individual mobile persons conform to the conditions of the aggregate in which they find themselves."³⁶ In a brilliant essay, Richard E. Mitchell has shown how the small aristocracy that ruled the late Roman Republic actually promoted the upward movement of *novi homines* into the lesser magistracies that had to be created in the wake of Rome's expansion and overseas conquests, in the process buttressing the power of the old families.³⁷ In the United States, as I have noted elsewhere,

the inordinate wealth, status, and influence commanded by relatively few families in our midst is, if anything, strengthened by the availability of great numbers of able young men and women drawn from families other than their own to occupy positions in the swelling bureaucracies of government and business, to do the scientific and social thinking, to dream the poetic dreams, to compose and sing the songs, to play heroically the games that are so vital to order and stability in modern society.³⁸

While there can be no question but that most wage earners do better as they get older, there is much question that age has had the crucial effect on wealth distribution that has recently been claimed for it. This argument relies almost entirely on aggregate data indicating that the mean average wealth of income earners of a given age category—say 21 to 30, 31 to 40, or 41 to 50—becomes greater as they get older. Its fatal statistical flaw is its indifference to the actual distribution *within* each age group, the likelihood that the higher average wealth of successively older groups masks a marked inequality among the members of each group and may be due primarily to the few super rich, who become very much richer as *they* get older. Its historical flaw is its indifference to the many factors other than youth that historians have shown actually accounted for poverty in American history, among the mature and the aging as well as the young. For, as the noted statistician Samuel B. Richmond has observed, "We don't care about the infinite number of models . . . that can be created or plucked out of thin air to yield distributions purporting to show that age accounts for inequality. *Our greatest interest is in how the distributions we have discovered got to be what they were.*"³⁹ Cliometric models

Fortune, 46 (1952): 132–35, 232–36; W. Lloyd Warner and James C. Abegglen, *Occupational Mobility in American Business and Industry, 1928–1952* (Minneapolis, 1955); Jocelyn Maynard Ghent and Frederic Cople Jaher, "The Chicago Business Elite, 1830–1930: A Collective Biography," *Business History Review*, 51 (1976): 288–328; and Ingham, *The Iron Barons*.

³⁶ Hope, "Introduction," in Hope, ed., *The Analysis of Social Mobility: Methods and Approaches* (Oxford, 1972), 8. Also see Barber and Barber, *European Social Class: Stability and Social Change* (New York, 1965), 5; C. Wright Mills, *The Power Elite* (New York, 1956), 53; and Judah Matras, "Social Mobility and Social Structure: Some Insights from the Linear Model," *ASR*, 32 (1967): 608–14.

³⁷ Mitchell, "The Aristocracy of the Roman Republic," in Frederic C. Jaher, ed., *The Rich, the Well Born, and the Powerful: Elites and Upper Classes in History* (Urbana, Ill., 1973), 27–63.

³⁸ Edward Pessen, "Social Mobility in American History: Some Brief Reflections," *Journal of Southern History* [hereafter, *JSH*], 45 (1979): 179–80. As for the composers of popular songs, I have subsequently discovered that the great majority of the successful writers were born to upper-middle-class families; Pessen, "The Great Songwriters of Tin Pan Alley's Golden Age (Between the Two World Wars): A Social, Occupational, and Aesthetic Inquiry," paper presented at the Ninety-Sixth Annual Meeting of the American Historical Association, held in Los Angeles, December 28–30, 1981.

³⁹ Richmond, as quoted in Edward Pessen, "The Distribution of Wealth in Antebellum America: Some Brief Reflections," paper presented at the annual meeting of the Social Science History Association, held in Cambridge, Mass., November 2, 1979.

throw no light on the historical factors accounting for inequality. Historical evidence indicates that many factors appear to have played a much more decisive role than age differences in accounting for poverty.⁴⁰

As for the inspired argument that high place in America has gone to the deserving and the best, the best that can be said for it is that it is impossible definitively to prove or disprove. The problem is that evidence bearing on this matter must be subjectively evaluated by each researcher. A common fallacy is to believe that a successful person born to rich and powerful parents must owe his own adult good fortune to the accident of birth. But this idea does not follow. It is possible that he or she may have succeeded in the absence of inherited advantages. Correlations do not causal connections establish, not even correlations between successful individuals and their privileged beginnings. The only way to ascertain the comparative extent to which native ability, industriousness, accident, and artificial or inherited material and social advantages contributed to someone's success is to gather evidence on the individual, the richer and more detailed and nuanced the evidence the better. The rub is that such evidence is not easily come by and, when it is accessible, will be variously interpreted. All one can hope for is that it be interpreted sensibly. From my own reading of the life histories of thousands of successful Americans, I am drawn to the conclusion that inherited social and economic advantages explain their careers far better than do inherited intelligence and character. In the understated language of a recent sociological study of the question, the stratified American social order "cannot be understood solely in terms of the personal characteristics of the individuals themselves."⁴¹

In sum, the argument that the unequal American class structure rests on a fair or equitable foundation, under which most individuals significantly improve their lot or get their just desserts, does not appear to be borne out by the historical evidence amassed to date.

The final question I shall ask about the American social structure, before examining its interaction with politics, concerns its impact on the lives of the American people. To judge from the neglect of class by American historians, the matter might seem of slight or peripheral significance, shedding little light on American life, of no great moment to the people themselves. Actually, it is hard to think of anything that comes near matching the effects that class has on the lives of most people or on the workings of American civilization. It is possible for people to profess indifference and to be sincerely oblivious to class or even to deny its existence and yet live lives that are largely shaped by it and by their membership in one class rather than another. Substantial research on American society from the Revolutionary era to the present establishes the manifold and powerful effects of class. Let me try briefly to synthesize these findings.

⁴⁰ Eric R. Nelson *et al.*, "The Measurement and Trend of Inequality: Comment," *AER*, 67 (1977): 487-531; Soltow, *Men and Wealth in the United States*, 61, 105, 175; Edward Pessen, "On a Recent Cliometric Attempt to Resurrect the Myth of Antebellum Egalitarianism," *SSH*, 3 (1979): 208-27; Michael B. Katz, "Social Class in North American Urban History," *JIH*, 11 (1981): 594-95; and Edward Pessen, "The Beleaguered Myth of Antebellum Egalitarianism: Cliometrics and Surmise to the Rescue," *SSH*, 6 (1982): 111-28.

⁴¹ E. M. Beck *et al.*, "Social Stratification in Industrial Society: Further Evidence for a Structural Alternative," *ASR*, 45 (1980): 713.

Class has controlled the quality and the quantity of the food people eat, the clothes they wear, their household furnishings and other possessions, the attractiveness of their homes and neighborhoods, the very air they breathe, the extent of their leisure and the kind of uses to which it is put, whether or not they have to work and whether their work is fatiguing, demoralizing, and irksome or attractive and fulfilling. It determines the social universe within which they move and marry and is an "important determinant of [their] sexual behavior," the quality of their marriages, and their fertility. The quality of education and the "socialization experiences" of children have been found to vary by social class, resulting in "different characteristics in children from different social classes" and ultimately in different positions in adult society. Sociologists exploring the hidden as well as the discernible consequences of class in the twentieth-century United States report that "the comparatively higher rates of [emotional and psychological] distress in the lower strata . . . is one of the most consistently documented findings in the literature on psychiatric epidemiology." Class has much to do with the diseases people are likely to suffer and the remedies, whether medicine, hospitalization, rest cures, or other, that they are able to afford. "Strong individual and aggregate level correlations between [class] and official measures of crime are among the most firmly established of social science generalizations"; class influences too the kind of crimes people commit, the quality of legal defense they can obtain, the severity of the punishments they are likely to receive. And class has affected not only the quality of people's lives but how long they have lived. As a student of early nineteenth-century Philadelphia reports, "Philadelphians not only held . . . uneven shares of life's pleasures, they also held uneven slices of life itself." One hundred and fifty years later, class continues to affect human longevity.⁴²

Several historians have recently argued that scholars who specialize in social

⁴² These points can be documented by a note of gargantuan proportions, since my thinking has been influenced by the fiction of Cooper, Melville, Wharton, William Dean Howells, Crane, Henry James, Jack London, Dreiser, Upton Sinclair, Sinclair Lewis, Fitzgerald, Thomas Wolfe, Dos Passos, Steinbeck, Marquand, and Louis Auchincloss, among others, by such contemporary treasures as the accounts by John H. Griscom and Lemuel C. Shattuck of the lives of the poor, the diaries of Philip Hone and Sidney George Fisher, the nineteenth-century working-men's press, the accounts of American life written by such visitors as Frances Trollope, Harriet Martineau, Thomas Hamilton, and Francis Grund, and by the biographies of men successful in many fields of American life. A selective list of relatively recent scholarly literature (which includes all the sources for the textual quotations) includes Leonard W. Labaree, *Conservatism in Early American History* (Ithaca, N.Y., 1960); Charles E. Rosenberg, *The Cholera Years: The United States in 1832, 1849, and 1866* (Chicago, 1962), and "Social Class and Medical Care in Nineteenth-Century America: The Rise and Fall of the Dispensary," *Journal of the History of Medicine and Allied Sciences*, 29 (1974): 32–54; Gerald N. Grob, *Mental Institutions in America: Social Policy to 1875* (New York, 1973); John Duffy, *A History of Public Health in New York City* (New York, 1968); John H. Ellis, "Businessmen and Public Health in the Urban South during the Nineteenth Century: New Orleans, Memphis, and Atlanta," *Bulletin of the History of Medicine*, 40 (1966): 407–29; Michael B. Katz, *The Irony of Early School Reform* (Cambridge, Mass., 1968); Ronald Story, "Harvard Students, the Boston Elite, and the New England Preparatory System, 1800–1870," *History of Education Quarterly*, 15 (1975): 94–121; Stuart M. Blumin, "Church and Community: A Case Study of Lay Leadership in Nineteenth-Century America," *New York History*, 56 (1975): 393–408; Michael S. Franch, "The Congregational Community in the Changing City, 1840–1870," *Maryland Historical Magazine*, 7 (1976): 367–80; Wendell Tripp, ed., *Through "Poverty's Vale": A Hardscrabble Boyhood in Upstate New York* (Syracuse, N.Y., 1974); Baltzell, *Philadelphia Gentlemen*, and *The Protestant Establishment*; Edward Pessen, "Philip Hone's Set: The Social World of the New York City Elite in the 'Age of Egalitarianism,'" *New-York Historical Society Quarterly*, 56 (1972): 285–308, and "The Lifestyle of the Antebellum Elite," *Mid-America*, 55 (1973): 163–83; Richard A. Berk and Kenneth J. Lenihan, "Crime and Poverty: Some Experimental Evidence from Ex-Offenders," *ASR*, 45 (1980): 766–86; John Hagan et al., "The Differential Sentencing of White Collar Offenders in Ten Federal District Courts," *ibid.*,

mobility research may have imposed their own "middle-class values," stressing the positive good in upward social movement, on working-class families that ostensibly were indifferent to such a movement.⁴³ In view of the thinness of the evidence presented in support of this thesis, one wonders whether the critics are perhaps imposing their own romantic values on working people.⁴⁴ None of this is to say that workers or other people in America have thought about their lives primarily in terms of class. No one knows precisely what people were thinking. The point is that, regardless of what may have been on their minds, every important aspect of their lives appears to have been significantly affected by class.

In sum, then, for all the changes that have overtaken it with the passage of time and for all its diversity at any particular time, a strikingly stratified class structure has always been a central feature of the American social order. And the quality of the lives lived by American families has been profoundly shaped by the levels they occupy in the class structure, levels whether high, middling, or low, that in many instances appear to have been attained by individuals for reasons other than their merit or lack thereof. These facts, as I shall try to show in the sections that follow, have profoundly affected American politics.

THAT THERE IS A CONNECTION OF SOME SORT between class and politics in America is close to a truism. The question is, What has been the nature of the connection since the federal Constitution first was drafted? Charles A. Beard's interpretation that the Founding Fathers or the framers of the Constitution sought above all to safeguard the "personalty" interests that they in their own holdings and investments represented has been attacked both for the narrow economic motives critics said it imputes to the Constitution's drafters and for its reading of their actually diverse economic involvements.⁴⁵ What is not in question is that the authors of the Constitution, whatever their motives and however complex their economic inter-

802–20; Ronald C. Kessler and Paul D. Cleary, "Social Class and Psychological Distress," *ibid.*, 463–78; Alan C. Kerckhoff, *Socialization and Social Class* (Engelwood Cliffs, N.J., 1972); Albert J. Mayer and Philip Hauser, "Class Differentials in Expectation of Life at Birth," in Bendix and Lipset, *Class, Status, and Power*, 281–84; Martin S. Weinberg and Colin J. Williams, "Sexual Embourgeoisement? Social Class and Sexual Activity, 1938–1970," *ASR*, 45 (1980): 33–48; Michael B. Katz and Mark J. Stern, "Fertility, Class, and Industrial Capitalism: Erie County, New York, 1855–1915," *American Quarterly*, 33 (1981): 63–92; Jerold S. Auerbach, *Unequal Justice: Lawyers and Social Change in Modern America* (New York, 1976); Smith, "The Dawn of the Urban-Industrial Age," 309; and John Braithwaite, "The Myth of Social Class and Criminality Reconsidered," *ASR*, 46 (1981): 36–51. Braithwaite concluded, despite the implications of the title, that "class is one of the very few correlates of criminality which can be taken, on balance, as persuasively supported by a large body of empirical evidence"; "Myth of Social Class and Criminality Reconsidered."

⁴³ Bruce Cummings, "Reflections on Schurmann's Theory of the State," *Bulletin of Concerned Asian Scholars*, 8 (1976): 60; James A. Henretta, "The Study of Social Mobility: Ideological Assumptions and Cultural Bias," *Labor History*, 18 (1977): 165–78; Dawley, *Class and Community*, 165–66; and Katz, *The People of Hamilton*, 109, 136.

⁴⁴ The men with whom I worked as a metal worker and welder for four years before World War II, cared about women, bowling, and big league baseball, but, like the working people I have encountered in my research in labor history, they gave much thought to their wages, the kind of work they did, their housing and material possessions, the leisure available to them, the opportunities available to their children, and other matters associated with class. See Edward Pessen, "A Young Industrial Worker in Early World War II New York City," *Labor History*, 22 (1981): 269–81.

⁴⁵ The two most influential criticisms are Robert E. Brown, *Charles Beard and the Constitution: A Critical Analysis of An Economic Interpretation of the Constitution* (Princeton, 1956); and Forrest McDonald, *"We the People": The Economic Origins of the Constitution* (Chicago, 1958).

ests, were not socially akin to the small farming families that constituted the great majority of the population at the time. The men who came together in spring 1787 to write a new fundamental charter for the nation were representative rather of the lightly populated but extremely influential and relatively wealthy upper levels of the social order. Nor was this social unrepresentativeness in any sense a historical accident.

Most of the Founding Fathers were, of course, men of high achievement, known for their public contributions during the American Revolution and after. They were also known for other things, not excluding their worldly success, their love of public order, their attachment to the rights of property, and their antipathy both to attacks on private property and to granting political power to the men of no or modest property who might be expected to be sympathetic to such attacks. It is not to deny that the architects of the new Constitution were indeed men of varied motives, some of these high-minded and patriotic, to conclude that they were selected (by the state legislatures, not the people) in part because of the known sympathy the more prominent among them felt for maintaining the stratified class structure of the time and the institutions on which it rested and the like beliefs of others of them, similarly situated socially, were presumed to have. Nor did the performance of the Founding Fathers disappoint such expectations.

The Constitution they completed in September 1787 was and is, of course, an expansive and flexible document, touching matters that have no bearing, direct or indirect, on the nation's social structure. The framers did not think in terms of class alone. But, as their finished handiwork reveals, they were not oblivious to the calls of class. The Constitution did promote the interests of those situated on the upper levels of the class structure, while safeguarding what I have called the "bedrock institutions" of a capitalistic society that accounted for and were likely to help perpetuate the social and economic inequality characteristic of that structure.

Forbidding states from creating money and impairing the obligation of contracts and recognizing as the new charter did the existence of slavery and the rights of slavery expressed concern not only for property rights in the abstract but for the rights of that minority of creditors and successful men who had negotiated contracts and owned property in human beings. Prohibiting direct taxes on a basis other than population constituted insistence on a regressive tax policy hurtful primarily to persons and families of modest means. Strengthening the national government at the expense of state governments and requiring state legislation to conform to the new supreme law of the land was assurance against recurrence of the stay laws, the paper money issues, and other evil practices by which some state legislatures had haunted the dreams of creditors. The class bias of the Constitution was manifested, too, in several of its political features.

In saying nothing about suffrage requirements or officeholding qualifications on the state and local levels at a time when such requirements and qualifications were widespread obstacles to popular participation in government, the Constitution acquiesced in rule by the propertied that such restraints had been designed to ensure. The "stake-in-society" theory that guided the political thinking of the Founding Fathers was not a timeless and socially neutral principal after all but the partisan political philosophy of substantial property owners, fearful that others

than they could not be relied on to legislate soundly. And what was the system of indirect elections for both the Senate and the presidency but a device to confine the vote for these most powerful elective offices in the nation to large property owners alone? For that matter, the very existence of an indirectly elected national upper house, possessed of special powers over foreign affairs and ambassadorial and judicial appointments denied the lower house, was yet another expression of the new charter's social partisanship. An appointive judiciary holding office during good behavior was likely to be more independent-minded than a popularly elected judiciary, but it was more likely too to consist of atypically successful men of sound social beliefs. Beard may have been wrong about the motives of the Founding Fathers, but he appears to have been right in interpreting the new Constitution as an assurance to substantial property owners that they could be secure in their possessions. And, out of evident fear that men without property or with too little property would pose a threat to this security, the new charter restricted the political participation of such men within the narrowest feasible limits. The House of Representatives alone among the agencies of national government was to be elected by the people—or, more precisely, by those adult males among the general population who alone met the suffrage requirements of the states.

A strikingly brief and deceptively simple document, the subtlety and complexity of the federal Constitution have inspired the production of a massive literature on and a continuing debate about its meanings and implications. What does not seem to be in question is that it was created by men themselves belonging to the upper clusters of a socially stratified society and that, at the time of its adoption, it sought to reinforce the prevailing social order. It did so by providing legal safeguards for property rights and arrangements earlier entered into and by creating a political system likely to be controlled by men sympathetic to these rights and to arrangements underlying the nation's stratification.

As subsequent events have shown, the Founding Fathers were remarkably prescient. But they were not seers. The Constitution made no provision for political parties. Since shortly after the Constitution went into effect in 1788, when a Jeffersonian Republican party emerged during George Washington's first administration to challenge his domestic and foreign policies and a Federalist party arose to defend these policies, the struggle for control over state and national government has almost always been waged by two closely matched major parties. And the major parties have for almost two centuries exercised a near monopoly over elective and appointive office in the United States. The Republicans and the Federalists were succeeded shortly after the War of 1812, the Panic of 1819, and the Missouri Compromises of the early 1820s by the Jacksonian Democrats and their opponents, first the National Republicans and subsequently, when the National Republican coalition proved unable to make a strong showing in the South, by the Whig party. After the Whigs were torn apart by sectional issues in the 1850s, the Republican party came into being, and that party and the Democratic party have ruled American politics for the past century and a quarter. The dozens of minor or "third" parties that have regularly appeared have invariably been ephemeral and almost always pathetically weak organizations, incapable of challenging or upsetting

the monopoly the major parties have enjoyed over officeholding.⁴⁶ In examining the performance of the major parties, I shall look at their leadership and the policies they have pursued. The leaders are worthy of careful study because even a scholarly age grown suspicious of elitism must recognize the very great power the leaders exercised over these organizations.

A CONTROVERSY HAS ARISEN AMONG HISTORIANS as to precisely when, during the young republic's first half century, the major parties became "mass parties." Certainly, from the time of their beginnings they sought popular support in the political contests they waged for elective office and did so more strenuously on the progressive liberalization of the suffrage.⁴⁷ Scholars have painstakingly sought to determine the ethnic, racial, denominational, occupational, wealthholding, and other measurable characteristics of the constituencies of the great parties. While voters for the different major parties might indeed have a distinct social profile, a voluminous literature indicates that they had one significant characteristic in common: their slight influence over the affairs of the parties.⁴⁸ To locate the sources of control, power, and policymaking in the major parties, it is necessary to focus not on the rank and file but on the leadership. While students who stress the "accountability" of the parties explain that popular influence is great, if indirect, the fact remains that the leadership determines the extent, if any, to which the presumed wishes and interests of their mass membership will affect the parties' public behavior.

⁴⁶One could, of course, marshal a footnote of staggering proportions to document this brief summary. Since I believe none of the points catalogued here are in controversy, I shall refrain from citing any single source on the history of political parties in the United States.

⁴⁷An older conventional historians' wisdom held that the Federalists, as an ostensibly aristocratic group who regarded the people with a mixture of fear, disdain, and contempt, were a party in spite of themselves, not courting popular favor. For devastating refutations of this idea, see Norman K. Risjord, ed., *The Early American Party System* (New York, 1969); James M. Banner, *To the Hartford Convention: The Federalists and the Origins of Party Politics in Massachusetts, 1789-1815* (New York, 1970); David Hackett Fischer, *The Revolution of American Conservatism: The Federalist Party in the Era of Jeffersonian Democracy* (New York, 1965); Manning J. Dauer, *The Adams Federalists* (Baltimore, 1953); Richard Buel, Jr., *Securing the Republic: Ideology in American Politics, 1789-1815* (Ithaca, N.Y., 1972); John A. Munroe, *Louis McLane: Federalist and Jacksonian* (New Brunswick, N.J., 1973); Paul Goodman, *The Democratic-Republicans of Massachusetts: Politics in a Young Republic* (Cambridge, Mass., 1964); Alfred F. Young, *The Democratic Republicans of New York: The Origins, 1763-1797* (Chapel Hill, 1967); Norman K. Risjord, "The Virginia Federalists," *JSH*, 33 (1967): 517; and Carl E. Prince, *The Federalists and the Origins of the United States Civil Service* (New York, 1978).

⁴⁸According to Robert A. Dahl, the history of New Haven indicates that "the poor man is not likely to gain influence, but, if he does, somehow along the way he is no longer a poor man"; Dahl, *Who Governs? Democracy and Power in an American City* (New Haven, 1961), 245. The burden of the massive literature on party policymaking at all levels of organization is how thoroughly it has been dominated by entrenched leaderships. Party conventions that hammered out platforms were typically meetings whose agendas and operations were controlled by insiders. And, it need hardly be documented, party platforms are one thing, party policies another. Richard Jensen, much impressed with the extent of popular participation in the late nineteenth-century Midwest, has suggested that such participation "was more than an ideal, it was a reality." But the "manifestations" of popular participation that he cited, in addition to "the simple act of voting," were confined to attendance and raising uproars at parades, picnics, barbecues—that is, to the rituals rather than the policymaking of the party. Policy, as his own evidence shows, was controlled by the "professionals." Jensen, *The Winning of the Midwest: Social and Political Conflict, 1888-1896* (Chicago, 1971), 2-3, 208. Also see Lenski, *Power and Privilege*, 334-35; Angus Campbell et al., *The American Voter* (New York, 1960), 90-93; and Robert Michels, *Political Parties: A Sociological Study of the Oligarchical Tendencies of Modern Democracy* (Glencoe, Ill., 1915), 50.

Since they are doubtless as complex as any other men, a discussion of political leaders can take many forms. I have elsewhere attempted to evaluate the values, ideals, intelligence, and character of antebellum political leaders.⁴⁹ Considerations of space and the dictates of the theme of this essay suggest the wisdom of focusing primarily on one of their many facets—their social and economic traits. It is possible to speak with some assurance because few matters have been so thoroughly investigated by historians and, for the twentieth century, by political scientists and sociologists as the adult circumstances of political leaders. Abundant data have been accumulated on the social and economic standing of party managers and behind-the-scenes directors, the men of the “juntos,” “regencies,” or machines that have often run the parties on the state and local levels, and on candidates for and appointees to offices, high and low, ranging from the humblest village or country posts to the most exalted national positions.⁵⁰ The burden of the evidence is that the nation’s political leaders have been most unrepresentative of the people, coming as the leaders almost invariably do from the lightly populated upper-middle and upper classes. Nor did the disappearance of property requirements for officeholding alter the trend.

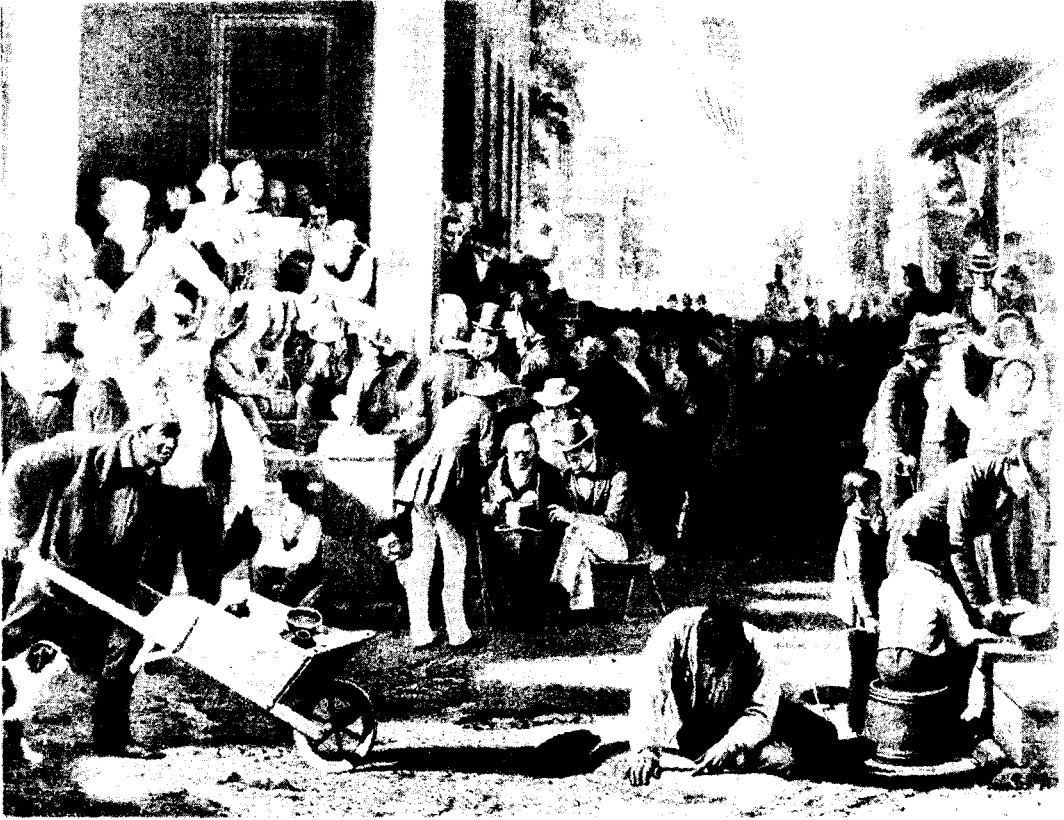
During the middle decades of the nineteenth century, state after state moved to banish the property and other restraints on officeholding that had earlier obtained. In the language of an eminent historian of two generations ago, “no longer was there . . . a theory that any class of men was wiser, abler, or better than another in the public business.”⁵¹ Actually, the theory that public office should be filled by the rich and the well-born appears to have lost favor before state constitutional amendments formally proscribed it. Yet, despite the popularity of the abstract notion suggested by Andrew Jackson in his first annual message that men of every station were capable of performing the duties of public office, during and after Jackson’s time, as before, office was contested for and occupied inordinately by the rich, the well-born, and the well-to-do of the upper-middle class. The democratization of officeholding requirements did not result in the social democratization of officeholders.

The evidence is, of course, not all of a piece. Inevitably, the patterns vary. The

⁴⁹ Edward Pessen, *Jacksonian America: Society, Personality, and Politics* (rev. edn., Homewood, Ill., 1978), chap. 9: “The New Men of Politics.”

⁵⁰ For discussion and cataloguing of the substantial literature on political leaders of the first two party systems, see two of my recent essays: “How Different from Each Other Were the Antebellum North and South?” *AHR*, 85 (1980): 1137–38; and “We Are All Jeffersonians, We Are All Jacksonians: or, A Pox on Stultifying Periodizations,” *Journal of the Early Republic*, 1 (1981): 22–25. A small sampling of the literature on the post–Civil War years includes Philip H. Burch, Jr., *Elites in American History: The New Deal to the Carter Administration* (New York, 1980); Vincent P. DeSantis, “The Politicos of the Gilded Age: A Social Analysis,” paper presented at the annual meeting of the Southern Historical Association, held in Atlanta, November 1977; G. William Domhoff, *Who Really Rules? New Haven and Community Power Reexamined* (Santa Monica, Calif., 1978), and *Who Rules America?* (Engelwood Cliffs, N.J., 1967); Harris, *Political Power in Birmingham*; Dahl, *Who Governs?*; Thomas R. Dye, *Who’s Running America? Institutional Leadership in the United States* (Engelwood Cliffs, N.J., 1976); David J. Rothman, *Politics and Power: The United States Senate, 1869–1901* (Cambridge, Mass., 1966); Donald R. Matthews, *The Social Backgrounds of Political Decision-Makers* (New York, 1954); and Kenneth Prewitt and Alan Stone, *The Ruling Elites: Elite Theory, Power, and American Democracy* (New York, 1973).

⁵¹ Dixon Ryan Fox, “New York Becomes a Democracy,” in Alexander C. Flick, ed., *History of the State of New York*, 6 (New York, 1933–37): 33. Fox was discussing the implications of the 1845 amendment to the New York State Constitution.



Courtesy of the State Historical Society of Missouri

Figure 2: Lithograph (1869?) after "The Verdict of the People" (1855), oil by George Caleb Bingham (1811–1879). Reproduced courtesy of the State Historical Society of Missouri, Columbia.

social profiles of candidates for local and county office have tended to be more modest than those of aspirants to more lofty state and federal positions. In cities the social position of councilmen and aldermen has usually been inferior to that of mayors. As an influential attorney in early twentieth-century Birmingham put it, "The aldermanic position is one of minor importance; the honor does not, as a rule, appeal to the best class of our citizens."⁵² Philip Hone and his upper-crust set said very much the same thing seventy-five years earlier. In Robert A. Dahl's New Haven, industrial entrepreneurs by the mid-nineteenth century were replacing the urban patriciate at the head of government. In Hone's New York City, as in other cities of the Northeast, elite families that continued to seek the mayor's office had begun to stand aside from humbler municipal posts. In antebellum Natchez, in contrast, the rich and elite had at no time "offered themselves," finding it unnecessary in view of the deference shown them and their interests by officeholders. In the industrial era of the past century, professional politicians, more often than not of the upper-middle station, have increasingly replaced socially eminent men in local government, no doubt drawn, at least in part, by the lucrative opportunities local power makes available to able amorlists.⁵³ Members of the

⁵² Harris, *Political Power in Birmingham*, 82.

⁵³ Harold Zink, *City Bosses in the United States: A Study of Twenty Municipal Bosses* (Durham, N.C., 1930).

lower house of Congress, for all their uncommon worldly success prior to their election, have not matched the tone of the Senate, which at different times, including the present, has rightly been dubbed a “millionaire’s club.” Nor do these exhaust the internal variation among political leaders that a more leisurely study of the massive evidence might disclose—variations induced by time and by the dissimilar reputations, influence, and appeal of the nation’s myriad offices to successful men. None of these variations alters the fact that party leadership and officeholding at all levels and at all times has represented not the entire social structure but only its upper clusters. In T. B. Bottomore’s summary, “a rich man may have difficulty in entering the kingdom of heaven, but he will find it relatively easy to get into the higher councils of a political party or into some branch of government.”⁵⁴

Why is this so? And what does it signify? These questions have been surprisingly neglected. For all their heroic efforts in gathering evidence on the social and economic circumstances of party leaders, scholars—Marxists aside—have said very little about the implications of the evidence they have unearthed. Beyond noting that the data disclose the social and economic atypicality of party leaders or reveal that the leaders of party A are richer or poorer than those of party B, we have barely exploited these fruits of our research. Donald R. Matthews’s complaint, registered a quarter of a century ago, that “no completely satisfactory” work has been done on the relationship between the social backgrounds and the political beliefs and conduct of policymakers continues to have force.⁵⁵ On the assumption that a discussion that is less than “completely satisfactory” may nevertheless be useful, I shall offer some answers to the questions posed at the beginning of this paragraph.

The privileged social and economic coloration of American political leaders does not appear to be a historical accident. A statistically inexplicable roll of the political dice cannot explain the unrelenting nonrepresentation of the numerically predominant elements in American society. Paradoxically, in choosing leaders and finding them among men of uncommonly high standing, the major parties in my judgment have not chosen them primarily because of that standing. In selecting candidates for office they appear to have been guided above all by two considerations: the likely appeal of these candidates to voters and their ideological soundness.

The great number of unattractive candidates the parties have offered for high office suggests that the latter consideration may be more important than the former. A two-party system, in which each major party is aware that its opponents choose candidates by rules or a formula similar to their own, provides assurance to caucuses and nominating conventions that the often unlovely nominee they put forward is not likely to be at a competitive disadvantage, since he will probably be quite similar in his unimpressiveness to the mediocrity offered by their opponents.⁵⁶ In seeking what I call soundness—or a working philosophy fundamentally

⁵⁴ T. B. Bottomore, *Elites and Society* (Middlesex, 1964), 123.

⁵⁵ Matthews, *The Social Backgrounds of Political Decisions-Makers*, 38.

⁵⁶ According to Robert Michels, the “average leader” of the great vote-getting, pragmatic parties is rarely a moral giant; *Political Parties*, 205. As James Bryce observed almost a century ago, great men do not become major party candidates for president.

sympathetic to the social order—the major parties appear to have operated on the unspoken premise that soundness is most surely the ideological attribute of those who have thrived under existing arrangements. This contention is most strikingly sustained by the backgrounds of the men selected for important nonelective positions.

Appointees to high judicial, diplomatic, and administrative posts and to presidential cabinets appear to have been cut of even finer social cloth than have been the men who have had to run the electoral gauntlet.⁵⁷ A popular explanation for the selection of “dollar a year men” to executive agencies and multimillionaires to ambassadorial posts is that only rich men could bear the lack of salary in the one situation and the huge expenses of the other. Yet, it is a policy decision after all, not an iron law, that certain strategically important federal offices pay so little as to assure incumbency by the very rich alone. Nonpayment for office, whether in ancient Rome or Chartist England, was construed by knowledgeable observers as testimony not to public parsimoniousness but to a determination that the interests of wealth be directly represented on high. The same construction appears equally applicable to the similar practice when pursued by government in the United States. Is it possible that, in making such choices, presidents and their senatorial advisers have indulged a deference to the upper crust that they can express openly when their choices are, in effect, not subject to popular “veto”? It is possible too that their selectors in some instances simply thought rich men the best men. In view of the almost palpable mediocrity of many wealthy appointees, such a notion may testify more strongly to the ideological bias than to the acuity of those who cling to it.

Given the propensity of party leaders and office seekers in the era of democracy to speak of their humble origins and circumstances, it seems clear that they recognize the political gold in plebeian background. That they have steered clear of nominating and appointing men of such background indicates that principle, not “political gold,” figures most heavily in their thinking. The principle in this instance is that a philosophy of social conservatism is the most important requirement of a major party leader. Ironically, in assuming that worldly successes are most likely to be of sound ideological mind, the major parties implicitly agree with social radicals, whose philosophy they otherwise abhor, that men’s beliefs are shaped above all by their material and social circumstances. And not only economic determinists subscribe to this view; it also continues to command popular support.⁵⁸ Even some sophisticated scholars have not forsaken it.

⁵⁷ See Ralph A. Wooster, *The People in Power: Courthouse and Statehouse in the Lower South, 1850–1860* (Knoxville, Tenn., 1969), and *Politicians, Planters, and Plain Folk: Courthouse and Statehouse in the Upper South, 1850–1860* (Knoxville, Tenn., 1975); Sidney H. Aronson, *Status and Kinship in the Higher Civil Service: Standards of Selection in the Administrations of John Adams, Thomas Jefferson, and Andrew Jackson* (Cambridge, Mass., 1964); Kermit L. Hall, “The Children of the Cabins: The Lower Federal Judiciary, Modernization, and the Political Culture, 1789–1899,” *Northwestern University Law Review*, 75 (1980): 423–71; Michael U’seem, “Route to the Inner Circle,” *New York Times*, January 4, 1981, sect. F, p. 22; and Burch, *Elites in American History*. Burch’s book is a heroically researched study of the backgrounds of almost all men appointed to cabinet, judicial, and ambassadorial positions from the Washington to the Carter administrations, and his data are immensely valuable for all the unimpressiveness of his discussion of their significance.

⁵⁸ In the “Call In” radio program conducted by President Carter on March 5, 1977, he was asked the

Just one generation ago, Paul F. Lazarsfeld and two collaborators stated that “a person thinks, politically, as he is socially. Social characteristics determine political preference.”⁵⁹ Few of us today would subscribe to so uncompromisingly deterministic and inflexible a definition. The “political preferences” of individuals appear to be shaped by many influences, some of them having nothing to do with their material or social circumstances. A more acceptable reformulation of Lazarsfeld’s thesis would be that major parties in America, in confining their selection of candidates for office almost entirely to persons high in the social structure, have done so on the assumption that such persons are likely to deal sensibly or soundly with issues bearing on social stratification.

The political predominance of the upper levels of the class structure no doubt has multiple explanations. Some political scientists have argued that prosperous men already leaders in politics are simply most comfortable with persons of their own sort, therefore choosing future leaders of similar standing. The expense of waging a campaign for office, whether in the Virginia of Washington’s time or the West Virginia of Kennedy’s, is another explanation. Yet another is the admiration Americans seem to feel for men who achieve wealth and high station and the evident popular acceptance of the idea that meeting a large payroll or achieving commercial success in producing, marketing, or investing in a commodity is somehow a useful clue to an individual’s statecraft and capacity for political leadership. What these explanations have in common is their implication that upper-class predominance in major party leadership has come about for reasons having nothing to do with the interests or ideological preferences of these classes.

None of these explanations is satisfying. It is questionable that successful men engaged in politics seek “comfort” from their involvements and their companions in these involvements in the sense that they seek it in the exclusive social clubs that they form and join. As for the need for money, he who is not himself rich may be capable of attracting heavy financial support from others, who find his character—or lack thereof—attractive, thus discounting personal riches as a requirement for political success. And one suspects that were the major parties inclined to do so, they would have little difficulty in making highly intelligent and learned candidates of modest circumstances at least as acceptable to voters as is success in financial speculation. Plato’s brilliant argument in *The Republic* could be used to good effect to accomplish this purpose.

I would make one additional point before proceeding to discuss major party policies—or, to put it more precisely, policies *in addition to* their policy of selecting political leaders from the upper social clusters alone. I agree with those who hold that the most important thing to be known about political actors is their public performance and the implications and likely consequences of that performance.⁶⁰

following question by Mrs. Esther Thomas, who described herself as a Republican from Villanova, Pennsylvania: “How can we as middle-class wage earners expect legislation or reform that would remove tax loopholes the rich or affluent use as deductions, when all laws and legislation are made by the rich?” She continued, “There are no poor people, no lower-class wage earners in either the House or the Senate.” *New York Times*, March 6, 1977, sect. A, p. 30.

⁵⁹ Lazarsfeld *et al.*, *The People’s Choice* (New York, 1948), 27.

⁶⁰ Likely consequences are, alas, less precise and harder to measure than actual consequences. But they are

Their backgrounds, whether emotional or social, interest us primarily insofar as they may help explain their actions. Yet, the almost invariable selection by the major parties of worldly successes as candidates for office, as for party management, was itself a significant political act, the important likely consequence of which was to ensure that political power would be wielded by men sympathetic to the institutions and arrangements underlying the stratified class structure. Such sympathy was not merely to be inferred from the careers and circumstances of those who were selected for leadership; a successful and wealthy individual is perfectly capable of harboring radical and iconoclastic beliefs about inequality. The men chosen for party leadership were usually not unknowns, certainly not to those who singled them out for leadership. They had usually demonstrated that they understood and knew how to play the game, before they were tapped for party honors.

What do the policies pursued by governments or the parties in power reveal about the interrelationship between politics and class structure in America? One no doubt fruitful way of trying to answer this question would be patiently to examine the circumstances surrounding every legislative enactment, administrative edict, and judicial decision that directly or indirectly seems likely to have affected the social structure, no matter how slightly, for evidence on the origin of these measures, rulings, and findings, the social composition of the lineup for and against them, their likely consequences at the time of their passage, promulgation, or defeat, their subsequent actual consequences, and whatever light the gulf between anticipation and actuality throws on the reciprocity between class and politics.⁶¹ Libraries remain to be filled with the results of future research inspired by this painstaking approach. I shall follow a more general and more circular path of inquiry, which is perhaps more appropriate in a relatively short essay. It is based in the first instance on several general perceptions about the American political system and how it operates.

Paralleling the absence of ideological cohesion among the highest social clusters is what I believe to be the typical absence, among the nation's most influential political figures, of any sense that their purpose is to promote the interests of the upper classes. America's major parties have not been at the end of a string that is manipulated by powerful capitalists; the often discordant and jarring tunes played in American politics have not been called by powerful elitists, for all their paying a

also more germane. For no one can foresee the actual consequences of a political act; when unanticipated, as they usually are, they throw no light on what was on the mind of those who performed the act. In trying to understand what a politician is up to when he votes or acts in a certain way, we have no sensible alternative, apart from "smoking-gun" evidence that clearly establishes his motives, but to make a subjective judgment, based on best evidence, as to what someone with his knowledge of events and in his circumstances had reason to expect would probably follow from his behavior. Such a judgment, I hardly need add, can never be proven right. It is enough that it can be informed and sensible.

⁶¹ Interestingly, political scientists and sociologists of the "pluralist" persuasion have used an approach very much like the one I have described, but in their case, of course, they use it to locate the distribution of power within communities. As the work of sensible scholars, interested in publishing during their own lifetimes, the books and articles reporting their research typically focus on a small number of issues and bills, studied in great depth. See, for example, Dahl, *Who Governs?*; Nelson W. Polsby, *Community Power and Political Theory* (New Haven, 1963), and "How to Study Community Power: The Pluralistic Alternative," *Journal of Politics*, 22 (1960): 474-89; Arnold Rose, *The Power Structure* (New York, 1967); and Seymour M. Lipset, *Political Man: The Social Bases of Politics* (New York, 1960).

large part of the bill. To understand the American political system is to understand its large element of spontaneity and the freedom enjoyed by men in office to chart out their own paths, free from dictation by forces of wealth and privilege. That more than one corrupt alderman, mayor, state assemblyman, U.S. senator, cabinet member, and even president have indeed danced on a string manipulated by a wealthy "benefactor" constitutes the exceptions that prove the rule. The American rich and social elite have too often gone unconsulted or had their wishes overridden in matters of high importance to them to justify any notion that the state and those who occupy it are merely puppets of the ruling classes.⁶² To give them their due, Andrew Jackson and Theodore Roosevelt (to name two individualists) were no man's creatures. The point is not that the nation's leaders have been indifferent to buttressing the prevailing social structure but rather that they have neither taken nor been required to take dictation from others in contemplating what political policies they would pursue.

The autonomy and spontaneity of the American political system as well as the occasional opposition by those in office to the perceived interests of the rich testifies not to the lack of power of the upper classes but to their realism and shrewdness. To use Tocquevillean phrasing, in predemocratic times great merchants and landowners could sit in assemblies, councils, and governors' chairs, using office frankly to enlarge their estates and otherwise promote their interests, answerable to themselves alone.⁶³ Political democracy in America, as the young Marx noted, seemed to make all things possible, including the spoliation of the upper orders by the lower.⁶⁴ In the debates held in the state constitutional conventions that were called during the early nineteenth century to consider the democratization of the suffrage and officeholding, speakers opposed to abolition of the property requirement regularly recalled the excesses, the confiscations of private wealth by the masses in Antiquity, warning that such scenes might be re-enacted in a democratic America. These fears proved groundless. For, in Henry Clay's time as in our own, the great majority of voters had, in the phrase of Paul Lazarsfeld and Robert K. Merton, been rendered "conformative to the social and economic status quo."⁶⁵

⁶² As Irwin Unger has shown, the opposition of the nation's "banking fraternity" to the National Banking Act did not prevent its enactment, any more than the subsequent opposition of "postwar business interests" prevented Republican politicians from promoting currency policies that served their own immediate political interests; Unger, *The Greenback Era: A Social and Political History of American Finance, 1865–1879* (Princeton, 1964), 15, 25, 44, 263. Historians have convincingly shown that many, if not most, of the nation's wealthy business interests favored neither the Civil War nor the Spanish-American War. See Philip S. Foner, *Business and Slavery: The New York Merchants and the Irrepressible Conflict* (Chapel Hill, 1941); and Julius W. Pratt, *Expansionists of 1898* (Baltimore, 1936).

⁶³ Labaree, *Conservatism in Early American History*.

⁶⁴ Marx in 1843 regarded the attainment of the democratic suffrage as the "political emancipation" of the masses or "great progress," which was "the final form of human emancipation within the framework of the prevailing [capitalistic] social order"; Marx, "Bruno Bauer, *Die Judenfrage*," in Marx, *Early Writings* (New York, 1963), 12, 15.

⁶⁵ Lazarsfeld and Merton, "Mass Communication, Popular Taste, and Organized Social Action." The complex process by which people learn to accept their lowly place and act as though they are indifferent to the transformation of the social order in their own interest is outside the scope of this essay. The problem is, of course, related to the classic question recently posed again by Seymour M. Lipset; Lipset, "Why No Socialism in the United States?" in Seweryn Bialer and Sophie Sluzar, eds., *Sources of Contemporary Radicalism* (New York, 1977), 31–149, 346–63. Lipset's essay usefully summarizes the main points made in the vast literature on the theme. My own recent contribution to the debate goes beyond my earlier argument: "Why Aren't American

Class war and parties and organizations advocating it had no charms for the mass of Americans of the lower orders—which is not to say that the major parties cheerfully invited the masses into positions of leadership and policymaking. In Martin Van Buren's era as later, masterful insiders operating behind the scenes proved as capable of manipulating and controlling the democratically elected nominating conventions that became the rage after 1830 as they had previously controlled caucuses or a selection process confined to party leaders. The major parties might strive to attain the widest possible voting support, but, as I have tried to show, they effectively insulated policymaking against the influence of the masses.⁶⁶

The great concession America's upper classes made to political reality in the democratic era was to forego direct control over and selfish class exploitation of political power, thereby running the risk of intermittently suffering stunning defeats. But these defeats did not jeopardize the stratified class structure or the upper crust's envied place in it. For under the implicit bargain struck by the upper classes and those, largely from their own ranks, who dominated the parties that governed, the essentials of the social order were to be left intact, for all the harshness of the rhetoric politicians might sometimes direct against its cruder inequities and for all the legislative defeats the propertied class would need occasionally to suffer. Of course, no meeting room ever witnessed the making of such a bargain. But the performance of the parties in power has not been inconsonant with the conclusion of such a contract. From the point of view of the upper crust, both their occasional defeats and their intermittent oratorical humiliation were advantageous. For they helped convince the electorate that politics and politicians are uniquely independent of upper-class influence in America. And what social teaching serves better the interests of a powerful but numerically slight class of men in a mass society than the teaching that they are powerless or possessed of no more power than any other group? Even the brilliant Tocqueville was taken in!⁶⁷

The necessity that major parties win popular electoral support has compelled them, in Dahl's language, to be "somewhat responsive to the preference of some ordinary citizens."⁶⁸ Since man lives not by bread alone, the parties in power have at times been responsive indeed to what some modern historians call "cultural issues" reflective of differences in the ethnic identities, the denominational ties and religious convictions, the value systems, and the lifestyles of their constituencies.⁶⁹

Workers Radical? A Classic Question Still Awaiting a Satisfactory Answer," paper presented at the annual meeting of the North American Labor History Conference, held in Detroit, October 9, 1981, and "Why the United States Has Never Had a Revolution—Only 'Revolutions,'" *South Atlantic Quarterly*, 72 (1973): 29–42.

⁶⁶ In Seymour Lipset's phrase, the "citizenry may . . . have a high rate of voting turnout, and yet have little or no influence on policy"; *Political Man*, 179.

⁶⁷ Tocqueville, *Democracy in America*, pt. 1: 60.

⁶⁸ Robert A. Dahl, *Preface to Democratic Theory* (Chicago, 1956), 131–32. For refutations that stress the absence of true or serious responsiveness by the major parties to the public's wishes and interests, see Prewitt, *Recruitment of Political Leaders*, 210, 316; and Richard F. Hamilton, *Class and Politics in the United States* (New York, 1972), 16.

⁶⁹ A sampling of the extensive literature on these themes includes Allan G. Bogue, "The New Political History in the 1970s," in Michael Kammen, ed., *The Past before Us: Contemporary Historical Writing in the United States* (Ithaca, N.Y., 1980), 231–57; Lee Benson, *The Concept of Jacksonian Democracy: New York as a Test Case*

Blue laws and laws calling for the closing of saloons, racial exclusion, school prayer, denial of women's rights, are no less dramatic, important to people, or capable of illuminating the values they live by than any other. The argument presented here is not that political issues bearing on the class structure alone are important. That other matters count too does not, however, diminish the significance of people's material circumstances or laws bearing on them. For that matter, what evidently was true of "ethnocultural" issues in mid-nineteenth-century Michigan may have been true elsewhere: "cleavages" such as these "probably insulated the social order from any serious challenge"; party programs focusing on such issues "never threatened the distribution in society of property or power."⁷⁰

Antitrust acts, inheritance laws, the establishment of regulatory agencies, the Sixteenth Amendment, the National Labor Relations Act, the social security system, the War on Poverty, Medicaid, welfare, and food stamps are among the panoply of measures that have no doubt "somewhat" altered the shape of the nation's economic and social structure. The great question is, How much have they altered it? How much were they designed to alter it?⁷¹ It does not reduce their significance to a nullity to construe these policies as the price those who support an inegalitarian social structure have of necessity paid to maintain that structure in the face of mounting popular expectations and appeals from the left for more drastic social transformation. That the price paid has not been terribly high seems clear from the continued thriving of monopolies, the retreat from the War on Poverty, and the recurrence of the plaintive query, "Who is going to regulate the regulators?"

In acquitting the Franklin D. Roosevelt administrations of responsibility for the persistence of widespread poverty right up to the eve of the Second World War, Richard Polenberg has concluded that "these conditions derived from a class system so deeply rooted . . . that it stubbornly resisted most efforts at change."⁷² To follow Polenberg's line of argument, the efforts at political change that the class system—or those in command of its higher levels—did *not* resist were unlikely to modify significantly, let alone undermine, the prevailing inequality. The American version of Disraelian and Bismarckean social welfare measures might induce near

(Princeton, 1961); Michael F. Holt, *Forging a Majority: The Formation of the Republican Party in Pittsburgh, 1848–1860* (New Haven, 1969); Ronald P. Formisano, *The Birth of Mass Political Parties, Michigan, 1827–1861* (Princeton, 1971); Jensen, *The Winning of the Midwest*; Samuel T. McSeveney, *The Politics of Depression: Political Behavior in the Northeast, 1893–1896* (New York, 1972); Paul J. Kleppner, *The Cross of Culture: A Social Analysis of Midwestern Politics* (New York, 1970), and *The Third Electoral System, 1853–1892: Parties, Voters, and Political Cultures* (Chapel Hill, 1979); Robert Kelley, *The Cultural Pattern in American Politics* (New York, 1979); and Samuel P. Hays, "New Possibilities in American Political History: The Social Analysis of Political Life," in Seymour Martin Lipset and Richard Hofstadter, eds., *Sociology and History: Methods* (New York, 1968), 181–227. For the chief flaws in the work done by this "ethnocultural" school, see Richard L. McCormick, "Ethno-Cultural Interpretations of Nineteenth-Century Voting Behavior," *Political Science Quarterly*, 89 (1974): 351–77; J. Morgan Kousser, "The 'New Political History': A Methodological Critique," *Reviews in American History*, 4 (1976): 1–14; Richard B. Latner and Peter Levine, "Perspectives on Antebellum Pietistic Politics," *ibid.*, 4 (1976): 15–24; James E. Wright, "The Ethnocultural Model of Voting: A Behavioral and Historical Critique," *American Behavioral Scientist*, 16 (1973): 653–74; and Pessen, *Jacksonian America*, 247–50.

⁷⁰ Formisano, *The Birth of Mass Political Parties*, 42–43, 55.

⁷¹ Documentation of these well-known points would be almost endless and seems unnecessary. On the ability of great wealthholders to deflect the impact of the higher inheritance taxes of the twentieth century, see Lampman, *The Share of Top Wealth-Holders in National Wealth*, 237–38.

⁷² Polenberg, *One Nation Indivisible*, 237–38.

hysteria in some overwrought and short-sighted conservatives but their passage, by further weakening the already forlorn movement for radical change in America, actually strengthened the class system.

Nothing attests more clearly to the political power commanded by the upper classes in America than the unwillingness of the major parties to undermine the stratified class structure or to entertain what Walter Bagehot called "bad issues" threatening to agitate the popular mind about the inequities of that structure. Since this observation rests on a conception of power that is not commonplace, let me elaborate on it.

For all their nuances and shadings of difference, most discussions of power emphasize the ability or capacity of those who possess it to have their way or "realize their will" in the face of opposition or to induce behavior in others that "conforms to the wishes" of the powerful.⁷³ The pluralist school of thought searches for the location of community power by examining the resolution of important issues that in effect have reached the agenda of government, to be voted up or down. Pluralism reigns, they conclude, because their close investigation of these political battles reveals that differing or shifting coalitions, rather than one entrenched group, have usually prevailed.⁷⁴ The pluralist methodology is far superior to that of the "elitist" interpretation they criticize, depending as the latter does on subjective and undocumented attributions of power to a covert elite by selected informants.⁷⁵ But the great weakness of pluralism is its assumption that the issues that come to pass, politically, are the truly important ones. In fact, the issues singled out by Dahl in his famous study of New Haven were likely to have little or no effect either on the class structure or on the lives of most people, whichever way these issues were voted. The pluralist model, as critics have noted (and as Dahl and his fellow pluralist Raymond E. Wolfinger have themselves conceded), "takes no account of

⁷³ In a perhaps tongue-in-cheek reaction to the multiplicity of definitions, Robert A. Dahl has said that power is "a concept that seems highly useful so long as one does not demand that it be defined"; Dahl, "Power, Pluralism, and Democracy: A Modest Proposal," paper presented at the annual meeting of the American Political Science Association, held in 1964, as quoted in Raymond E. Wolfinger, "Nondecisions and the Study of Local Politics," *American Political Science Review* [hereafter, *APSR*], 65 (1971): 1080. Useful discussions include Max Weber, *The Theory of Social and Economic Organization* (London, 1964), 152; Talcott Parsons, "Some Reflections on the Place of Force in Social Processes," in Harry Eckstein, ed., *Internal War* (New York, 1964), 57; Daniel Bell, *The End of Ideology* (New York, 1961), 47; Mills, *The Power Elite*, 91; Carl J. Friedrich, *Man and His Government: An Empirical Theory of Politics* (New York, 1963), 199–200; Marvin E. Olson, "Power as a Social Process," in Olson, ed., *Power in Societies* (New York, 1970), 3; Robert Perrucci and Marc Pilisuk, "Leaders and Ruling Elites: The Interorganizational Bases of Community Power," *ASR*, 35 (1970): 1040–57; James G. March, "The Power of Power," in David Easton, ed., *Varieties of Political Theory* (Engelwood Cliffs, N.J., 1966); and William Riker, "Some Ambiguities in the Nature of Power," *APSR*, 58 (1964): 341–49. A recent overview is David C. Hammack, "Problems in the Historical Study of Power in the Cities and Towns of the United States, 1800–1960," *AHR*, 83 (1978): 323–49.

⁷⁴ In addition to the pluralistic interpretations cited in note 61, above, see Unger, *The Greenback Era*, 405; Michael Kammen, *People of Paradox: An Inquiry concerning the Origins of American Civilization* (New York, 1972), 294; and Rothman, *Politics and Power*.

⁷⁵ The most influential "elitist" study is Floyd Hunter, *Community Power Structure: The Study of Decision Makers* (Chapel Hill, 1953). Also see Mills, *The Power Elite*; Domhoff, *Who Rules America?*; and Peter S. Bachrach and Morton S. Baratz, *Power and Poverty* (New York, 1970). Informed critiques of this approach include Dahl, "Critique of the Ruling Elite Model," *APSR*, 52 (1958): 463–69; Polsby, "How to Study Community Power"; and Raymond E. Wolfinger, "Reputation and Reality in the Study of Community Power," *ASR*, 25 (1960): 636–44.

the fact that power may be, and often is, exercised by confining the scope of decisionmaking to relatively 'safe' issues."⁷⁶ In view of the central importance to people of their position in the class structure and the amazingly diverse and significant effects that position has on their lives, the power of powers seems to be the "capacity" of the fortunate few at the apex of that structure to maintain their attractive and envied places, secure, as they have been, in the assurance that the major parties in control of government will safeguard both the system and the exalted place of the upper classes in it.

Power is, of course, an internally differentiated phenomenon. It takes a variety of forms, some more important than others, and, whatever shapes it assumes, power is unequally distributed among individuals and groups. Nor is power—and this is particularly true in a nontotalitarian society such as ours—confined to government. It is shared by political and nonpolitical bodies in some indefinable balance. The vast economic power wielded by the wealthy few and what might be called the social power of the upper-class men and women who monopolize offices and policymaking in influential voluntary associations and largely control the media seem beyond dispute. It is not clear that political power is more or less important than these other forms; different forms of power quite probably overlap and are interrelated. Precisely because all things ultimately are interrelated, the student who wishes to understand them is well advised to study them singly, in depth. In view of the transcendent importance to people of their place in the class structure, the *political* power of powers may be the evident capacity of those who command the most enviable places in that structure to be spared dangerous attacks on their privileged station by American governments.

IN ARGUING THAT THE FEDERAL CONSTITUTION made private property anterior to or beyond the reach of government, Beard was in effect saying that political bodies were subsequently prohibited from challenging the inequitable social structure by the nation's new fundamental charter and the insurmountable impediments it had put in place. In view of the welfare and taxation clauses and the Sixteenth Amendment, which was approved the same year that Beard's book was originally published, this argument is not compelling. A generation earlier, Henry George had suggested that a sufficiently stringent tax policy could achieve the benefits of socialism without having to pay its social costs.⁷⁷ Politics could have drastically rearranged the American social order in the interests of the mass of men who occupied its lower strata. That those who have controlled government have chosen not to promote such change, despite having been selected primarily by those below

⁷⁶ Peter S. Bachrach and Morton S. Baratz, "Two Faces of Power," *ASR*, 56 (1962): 947–52. For additional criticisms of pluralism, see Thomas J. Anton, "Power, Pluralism, and Local Politics," *Administrative Science Quarterly*, 7 (1963): 425–57; Andrew S. McFarland, *Power and Leadership in Pluralist Systems* (Stanford, 1969), 82–87; Frederick W. Frey, "Comments on Issues and Nonissues in the Study of Power," *APSR*, 65 (1971): 1081–1101; and Domhoff, *Who Really Rules?*. Dahl has conceded that in modern New Haven, the "most important economic and social goals" of the socioeconomic "notables" are "not often immediately at stake in local decisions"; *Who Governs?*, 78. And Wolfinger has suggested that it is indeed useful to study what most pluralists ignore: "what government fails to do"; "Nondecisions and the Study of Local Politics," 1079.

⁷⁷ Henry George, *Progress and Poverty* (Modern Library edn., New York, 1939), 328.

who would most profit from it, is the great paradox of American political history.

In trying to explain why this has been so, I have to this point argued that both by their public actions and inaction, major party leaders have demonstrated an ideological affinity for the prevailing social order, confirming the anticipations of those who had designated them for leadership largely on the assumption that they would be so disposed. In evaluating the policies pursued by major party leaders, I have stressed four points: their implicit agreement to agree not to take up publicly those issues the very discussion of which might stir up feelings threatening to the social order; their insistence on confining their choice of successors to men whose social and political philosophies and beliefs were likely to be similar to their own; their preoccupation with issues that, because they were peripheral to the "social question," were not threatening, however they might finally be decided; and their expectation that what they regarded as necessary reform measures would essentially buttress the social order by protecting it against its own abuses and worst tendencies. In effect, I have inferred the ideological conservatism of major party leaders from what they have chosen not to do and from the implications of several of their actions. Other of their political actions, even when swiftly glanced at, illustrate more directly the dedication of the nation's major party leaders to perpetuating the stratified class structure or catering to the interests of its dominant classes.

The public oratory of party leaders, for all its occasional demagoguery regarding this or that issue, has typically glorified the nation's social order and the equal opportunity for success it ostensibly offers all citizens. Such talk may indeed be what some skeptics call empty rhetoric, but it is a form of political action nonetheless—and a not insignificant action. That politicians' speeches are not to be taken literally does not mean they are not to be taken seriously. When highly admired leaders speak, many listen. To read at length in the public statements of major party politicians is to be immersed in platitude and vaguely populist rhetoric. One is reminded anew that demagoguery is the tribute paid by amorality to virtue in an age of democracy. Yet even opportunists have their frank moments. In going through Richardson's *Messages and Papers* and other repositories of presidential oratory, I have been struck by how clearly the speeches (and writings) of our very highest political officials reflect their sense of identification with the prevailing social order and the not always equitable institutions on which it rests. Whether reputed to be conservative, moderate, liberal, progressive, or radical, they have inculcated the beliefs that success and failure in life depend above all on an individual's ability and behavior, that inequality is inevitable, that life—not the social order—is unfair, that people in economic distress are apt to look too much to government, that the duty of government is to confer special benefits on no single group, that freedom and the continuation of freedom depend on the continuation of the prevailing social order, and that "the system," even at its worst, remains beneficent, requiring nothing more than the slightest tinkering.⁷⁸ In doing so they have helped

⁷⁸ I am at work on a manuscript on the interrelationship between the presidents' youthful and adult circumstances, their prepresidential careers, their social philosophies, and the policies of their administrations, in which I hope to deal with this matter in greater detail.

perpetuate the inequities in the social order. For few actions serve an institution so well as do the eloquent words of praise spoken in its behalf by the very men the mass audience, to whom these words are addressed, has long been taught to revere.

And much of what the major parties have legislated has served the upper classes not by indirection but directly, by giving them freely what they would have paid much money to get. The Nobel laureate in economics James Tobin has recently charged that the Reagan administration's economic policies are designed to widen the already great disparities in wealth, opportunity, and power between the rich few and everyone else. Similar charges might be made concerning an impressive catalogue of earlier acts and policies: imprisonment for debt under laws that were hurtful, above all, to poor and marginal debtors; "liberal" corporation and banking acts; a nineteenth-century "transformation of American law [that] actively promoted a legal redistribution of wealth against the weakest groups in society"; the nonenforcement of tariff laws, which forgave wealthy importers substantial interest payments; land giveaways and procorporate administration of public lands; a regressive tax system; the strange procorporate judicial interpretation of the Fourteenth Amendment that long obtained; antiunion labor policies; the harassment and even the suppression of radical or revolutionary movements and parties; and probusiness foreign policies—to name a few.⁷⁹ Further study is required to establish the extent to which this mournful catalogue of class bias is typical of American governmental behavior. Those performing such research should keep in mind that policies such as these are not necessarily "balanced out" by policies more bland, less flagrantly deferential to the upper orders.

The conservative ideological bias reflected in much governmental policy has indeed been interpreted by some critics as a sign that money was changing hands. In its more unsubtle manifestation, plutocrats bought individual senators and corrupted entire legislatures by pressing money into their not unwilling palms. More subtly, mammoth corporate campaign contributions to the coffers of the major parties helped induce a mood of receptivity to the interests of big business. David J. Rothman has offered the ingenious interpretation that large corporate campaign contributions at the turn of the century helped weaken the political influence of the corporations by making the parties "less vulnerable to outside pressures."⁸⁰ One wonders how long the mammoth contributions would have

⁷⁹ Peter J. Coleman, *Debtors and Creditors in America: Insolvency, Imprisonment for Debt and Bankruptcy, 1607–1900* (Madison, Wis., 1974), 267; Horwitz, *The Transformation of American Law*; Malcolm J. Rohrbach, *The Land Office Business: The Settlement and Administration of American Public Lands, 1789–1837* (New York, 1968); Roy M. Robbins, *Our Landed Heritage: The Public Domain, 1776–1936* (Princeton, 1942); Lenski, *Power and Privilege*; Lampman, *The Share of Top Wealth-Holders in National Wealth*; Barber, *Social Stratification*, 412; Polenberg, *One Nation Indivisible*, 22, 107; Peter H. Argersinger, "A Place on the Ballot: Fusion Politics and Antifusion Laws," *AHR*, 85 (1980): 287–306; Ruth Leacock, "Promoting Democracy: The United States and Brazil, 1964–68," *Prologue: Journal of the National Archives*, 13 (1981): 76–99; Lloyd C. Gardner, *Economic Aspects of New Deal Diplomacy* (Madison, Wis., 1964); Clayton R. Koppes, "The Good Neighbor Policy and the Nationalization of Mexican Oil: A Reinterpretation," *Journal of American History*, 69 (1982): 62–82; David Montgomery, *Beyond Equality: Labor and the Radical Republicans* (New York, 1967); and Herbert G. Gutman, *Work, Culture, and Society in Industrializing America* (New York, 1976).

⁸⁰ Rothman, *Politics and Power*, 6. To sustain this paradoxical interpretation, Rothman makes much of the fact that "corporate appeals for action were couched in terms of party welfare"; *ibid.*, 187. That, as Rothman seems to recognize, businessmen were shrewd enough to "couch" their selfish interests in acceptable terms is hardly a sign of their political impotence. Instructive on this point is Woodrow Wilson, *The New Freedom*, 57.

continued had the parties turned against business. I think that the important point is not that politicians' souls were actually bought by corporate monies but rather that major party leaders, for complex reasons I have suggested, acted as though this were the case.

The burden of the evidence does not sustain the charge that major parties and their leaders have been beholden to, because they were corrupted by, corporate money. Corporate monies have indeed flowed in significant quantity to the great parties. My interpretation of the transaction is that it has been undertaken more out of appreciation by the wealthy of the general services rendered by the major parties in preserving "the system" than in the expectation that party leaders will dance to this or that tune wealthy pipers have called. Such an understanding is perhaps more dismaying than corruption in its commonplace, venal form, for it suggests a kind of betrayal by public servants of the great implicit vow they swear to in a democracy to serve the interests of the many rather than the few.

THE NATION'S POLITICAL SYSTEM IS, in a sense, a parallel structure to its social order. Political leaders, themselves drawn from the upper levels of the class structure, owe their authority to the voting support and allegiance they receive from their rank-and-file constituencies, somewhat as upper classes owe their wealth and standing to the labor performed and implicit deference shown by the great majority who occupy the lower social levels. Led, as they have been, by men sympathetic to the fundamental arrangements underlying the nation's stratified class structure, the major parties in control of American government have acted as though their fundamental purpose is to reinforce this structure.

I suspect that this conclusion will leave many unconvinced. I concede this cheerfully, for my purpose is less to induce agreement than to provoke controversy. I hope that my observations stimulate further discussion of the important and relatively neglected theme of the interrelationship between class and politics in American history.

Comments:

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THE DISCUSSION THAT EDWARD PESSEN hopes to stimulate requires an understanding of what he has written. Although valuable suggestions do emerge from his article, they arrive with such a heavy camouflage of theory that perplexed readers might reasonably wonder what he really did intend to say. The first step toward a profitable exchange, therefore, involves stripping this disguise. At the outset, Pessen rejects the Marxist conception of class structure for its narrowness and rigidity, and throughout the article he repeats the apparently redundant phrase “stratified class structure” (see, for example, pages 1298, 1300) to reaffirm his opposition to the “Marxists’ two-level class structure” (page 1291 note 4). For an alternative conception he turns appropriately to the most fertile period of American anti-Marxist theory, covering the late 1930s to the late 1950s, and selects the six-tier scheme of W. Lloyd Warner and his associates, an incremental divider that spans a great range of distinctions in American society without cleaving it along any line. This model, Pessen concludes, most closely approximates the historical record of America’s social structure.

As Professor Pessen continues his general commentary, the Warner model grows exceedingly dim. Once he shifts to an examination of politics, it simply melts away, and in its place appears as cleanly separated a “two-level class structure” as any of those he has just repudiated. Lower-upper and upper-lower and the rest of Warner’s markers serve no function at all. What matters now are the rulers and the ruled. Throughout the national experience, Pessen argues, a small upper class has dominated the agencies of social power, regulated recruitment into these circles, and determined the agenda for public policy. The only enduring roots of this class have rested in wealth and property. Ideologically and institutionally, it has woven a seamless fabric over two centuries that has maintained cohesion on the inside while it has defied challenge from the outside. Before rising to positions of any importance, politicians have assimilated the upper-class world view. When legislative measures have contained the potential for substantial reforms, government administrators have neutralized them. No other nation’s ruling class in the past two hundred years, Pessen might have added, could claim an equally brilliant success.

Although Professor Pessen does not offer such a sweepingly smooth account, he leaves the summarizer with no other choice. Each of his qualifications to this monolithic rule dissolves into a false distinction or an inconsequential matter of detail. What useful meaning can the “autonomy” of politics have when its directors reflect so consistently the interests of the upper class? According to his own account, they have moved to those internal wires that no one ever has to pull and, even more important, that no one could ever break. After dismissing the existence of an

upper-class consciousness, Pessen then describes a very refined one that has responded to the need for indirect party rule at the critical moment and calculated the "price" for each of its sanitized reform measures. Only caricatures of class consciousness require a perfect harmony among its members. Hence, Pessen's examples of conflict between business and political leaders, set against two centuries of persistent political protection for the upper class, shrink into tactical squabbles inside a common, binding framework of values and purposes. On the one hand, he laments the absence of truly liberalizing reforms; on the other, he shows how flawlessly a shrewd and insulated upper class has parried all kinds of reform. The logic of Pessen's own analysis makes a revolutionary attack on America's class structure the one rational strategy for a significant redistribution of social rewards.

FROM THIS INITIAL DISJUNCTION BETWEEN THEORY AND ANALYSIS a series of difficulties spread to plague Professor Pessen's essay. First, by denying his affinity to Marxism, he has cut himself off from the very theoretical resources that do apply to his interpretation of a ruling class. Marxism offers precisely the definitions and explanations of power relationships that Pessen requires to give his article intellectual coherence and prepare it for serious discussion. Throughout his account of class and politics, for example, the writings of Antonio Gramsci simply beg for his attention. Moreover, the fleeting appearance of the Warner model casts a long shadow over the issue of how Pessen links local and national class structures. Warner and his associates designed their scheme to suit communities where family reputations, occupations, residential areas, and the like expressed roughly common meanings from common funds of community knowledge. By claiming to adopt these standards of familiarity and then generalizing about American society as a whole, Pessen leaves his readers wondering whether some comparable body of knowledge about families and residences might apply to a national upper class, whether national and local upper classes might have different characteristics, or whether a national upper class might be a mere fiction to cover innumerable, independent local elites. Although none of these alternatives fits his analysis, nothing else arrives to clarify the important issues behind them. Consequently, class becomes a category of convenience, a way for Pessen to designate a ruling power without obliging him to locate it firmly in a social structure at any level of American life.

To these problems stemming from theory, Professor Pessen adds a final one relating to history. Classes derive their qualities from particular patterns of historical circumstances. As these conditions change, so do classes. Yet with only slight shifts, Pessen's upper class lies frozen across the centuries, as if it had been impervious to almost everything that happened in the United States between George Washington's presidency and Ronald Reagan's.

To engage in a sensible discussion of Professor Pessen's article, historians need to know first of all where history fits into his scheme. When and why did a scattering of provincial elites form an American upper class? If, as Pessen indicates, that

congealing occurred some time before 1787, how did gentry rulers of the Revolutionary era sustain or transmit their power during a wholesale transformation of American society early in the nineteenth century? No portion of United States history poses more complicated problems in class analysis than the half-century from approximately 1825 to 1875. What means of coherence did an upper class have at its disposal? A class structure does not arise spontaneously from the parallel experiences of dispersed and otherwise preoccupied individuals. How were common values determined, let alone communicated and applied, during a time of such continual flux in business enterprise, demographic patterning, and territorial expansion? In what political arenas did a ruling class operate? During the second quarter of the nineteenth century in particular, the primary dynamic in economic politics ran between state governments and local groups or governments, and what ties interconnected America's myriad elites nationally appeared no more obvious then than they do now. Pessen's easy dismissal of local governments as beneath the concern of the ruling class seems most suspect during these years. Assuming an upper class did exist in the middle decades of the nineteenth century, how did this one survive America's next major transformation into the twentieth century?

In his account of the past hundred years, when the presence of an American upper class has much greater plausibility, other chronic confusions in Professor Pessen's sketch come to have more significance. How has the line been drawn between rulers and ruled? As the types of property, the sources of wealth, and their relations to social power have become matters of increasing complexity, allusions to the rich and the rest tell less and less about the ways in which crucial class distinctions have been made. At times, Pessen writes as if a commitment to property rights alone had constituted the culture of the ruling class. Although a few historians have tried to blend the Lockean consensus of Louis Hartz into a class analysis of American society, this merger does not pop immediately to mind as a self-evident proposition. Without a good deal of elucidation, the two seem at odds, as Hartz meant them to be. Nevertheless, if Pessen could manage such a combination, he might illuminate the puzzling passivity of the ruled, an issue too intimately involved in the continuity of the rulers to set aside as quickly as he does. Certainly, he cannot shelve it as "irony."

NO ONE, OF COURSE, EXPECTS Professor Pessen to resolve these problems in an overview. What historians can expect, however, is a clear view, one that reveals a sharp line between theory and evidence, shows the contours of an authentically historical interpretation, and highlights the actual structure of a changing society. Instead, Pessen has left the readers to fill in so many blanks that, in the end, they must create their own designs for a class analysis of American history.

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SOMETIMES A SINGLE BOOK is a landmark in the history of a field. Fresh and powerful, it points research in new directions, spawns dissertations, and stimulates debate for at least a generation. Above all, it is timely. In retrospect, its questions are immanent in the intellectual atmosphere, waiting for an artisan with the skill to pull them out and shape them into a new paradigm. We read a landmark book and we say, yes, this is the book for which I have been waiting. In the last two decades, there have been two landmark books in the history of social structure, one each for Britain and the United States. Both reflect not only the talents of the author but the national intellectual contexts as well. For Britain, the book is E. P. Thompson's *The Making of the English Working Class*; for America, it is Stephan Thernstrom's *Poverty and Progress*.¹ In contrast to America, the existence of a working class is not problematical in Britain, and the great tradition of British working-class history has long been part of the broader labor movement in British society. Although Thompson's work fits this pattern, its expansion of working-class history beyond trade unions and politics to a concern with culture and the lived experience of ordinary people redefined the questions for working-class historians and pointed them to fresh sources. His work has had an enormous impact on many areas of British historical writing, but it has especially inspired younger scholars dissatisfied not only with the usual boundaries of working-class history but with contemporary working-class politics as well. Although Thernstrom could not draw on a great tradition of American working-class history, *Poverty and Progress* reflects the re-examination of American life by social scientists during the 1960s. As they began to expose the structure and reproduction of inequality and the malfunctions of social institutions, scholars punctured some of the most cherished and useful American myths. (For Thernstrom it was the myth of equal opportunity.) In this way, social scientists buttressed the movement for social justice that flourished so intensely and briefly several years ago. My point here is very simple: the best recent social history has been very political. Only its questions have been different from those usually asked by political historians. For social historians to focus their energy on those questions—to use the power and insight of recent research simply to provide more documentation for an agenda set decades ago—would be like channeling all of one's political activity into traditional party politics. As the Civil Rights, Environmental, Antinuclear, and Women's Movements have taught us, politics has changed, and it is worth asking why. Unfortunately, Edward Pessen misses this essentially political character of much recent social history. Thus, his appeal for a closer relationship between social and political history is, in the last analysis, a call for footnotes to a story whose main outlines are already well known. The more interesting task is to write a new story.

WHAT PROFESSOR PESSEN REALLY WANTS, of course, is to convince political historians that they need to include class or social structure in their explanations. I seriously

¹ Thompson, *The Making of the English Working Class* (London, 1964); and Thernstrom, *Poverty and Progress: Social Mobility in a Nineteenth-Century City* (Cambridge, Mass., 1964).

doubt that any political historian would disagree, and I don't know against whom Pessen has directed his criticism. But leave that aside for the moment. Certainly, historians should look for better ways of interconnecting the recent exciting developments in social and political history. Indeed, Bernard Bailyn made exactly this point in his presidential address to the American Historical Association, published in this journal. The issue is not the need to inject politics into social history and social history into politics; it is the nature of the connection. Bailyn's address raises the level of conceptualization and points historians toward new ways of thinking about the inseparable nature of politics and society. So does Charles Tilly in his new book, *As Sociology Meets History*.²

Professor Pessen, by contrast, raises no new questions. By default, he defines politics as elections and the activities of elected officials. He argues against the idea that politicians and capitalists usually strike explicit bargains. Instead, he says that political candidates are chosen by men of wealth and power for their soundness and usually do not disappoint their sponsors. Thus, politics serves to perpetuate rather than change class structure. Probably true enough, but, again, is that all we can extract from the last fifteen years or so of American political historiography? I leave it to political historians to make detailed comment on Pessen's treatment of their field and its central concepts. Here I want to make only two observations prompted by this part of his essay.

First, politics consists of more than elections and the behavior of elected officials. There are struggles over power and resources, negotiations, and legislation in many arenas. Even family life has its politics. Perhaps more interesting than the background of office seekers would be the ways in which various political arenas overlap with each other and are affected by class relations.

Second, the place where social structure and politics come together most closely is in the analysis of the state. Charles Tilly claims, rightly I think, that the two great processes in modern history are proletarianization or the development of capitalism and the formation of nation states.³ The two are joined intimately and inextricably. Showing the nature of their connection and tracing the variation from place to place—this should be the view from the mountain of monographs by social and political historians. It is why, really, social and political history are part of the same quest.

Social and political history intertwine especially tightly in the work of a number of British historians. Professor Pessen, however, is not concerned with the history of social structure in Britain. As far as his historiography is concerned, it might just as well not exist. Fair enough, we might say, were American social-structural history not profoundly—and increasingly—influenced by developments in Britain. For the moment, though, let us overlook his neglect of E. P. Thompson, Eric Hobsbawm, John Foster, and others. But we cannot overlook the gap in his treatment of the issues raised by Thernstrom. Pessen is concerned with the historiography of social structure in America in the post-*Poverty and Progress* era. For that reason, he should

² Tilly, *As Sociology Meets History* (New York, 1981).

³ *Ibid.*, 191–209.

have read Thernstrom with great care. Now *Poverty and Progress* is notable in several respects. It showed how the history of ordinary laborers could be written, pioneered a technique for the historical study of social mobility, demonstrated the distinction between occupational and property mobility, uncovered the massive population movement in and out of nineteenth-century towns and cities, assessed the relation between ideology and social processes, and punctured a large hole in myths about a golden age of equal opportunity in mid-nineteenth-century America. But it did even more. It focused on Newburyport, Massachusetts, scene of a famous community study by W. Lloyd Warner and his associates in the 1940s. It showed how Warner's ahistorical approach led him to misinterpret contemporary Newburyport. In a stunning appendix Thernstrom took the offensive against contemporary social science. History is not a mere borrower of sophisticated concepts and methods, he argued. Rather, it is an essential component of social analysis and social theory. As he hammered home his case by shattering Warner, Thernstrom delivered a particularly devastating blow to the six-class scheme on which Warner's work was based. The reputational method used by Warner to divide the population of Newburyport into six classes (upper-upper, upper-lower, and so forth) had no theoretical basis, lacked any solid historical grounding, and was founded, in the last analysis, on gossip.

One of the ironies of Edward Pessen's essay is that he adopts Warner's six-class scheme as the most adequate description of American social structure, past (including the colonial period) and present. How does Pessen decide that a largely discredited formulation of 1940s-style American sociology remains so powerful? His logic is not easy to follow, but I think it goes something like this: (1) class and social structure are synonymous and interchangeable terms; (2) the debates about the definition of class are inconclusive and pointless; (3) therefore, all criteria should be incorporated in an ecumenical fashion to produce a flexible, multi-dimensional concept of class, a definition that eschews theory in favor of common sense (thus giving us Pessen's substitution of a list for a definition); (4) it just so happens that these multi-dimensions almost always form six rough clusters arranged in a hierarchical fashion; (5) the content of these clusters changes over time (for reasons that he leaves largely unspecified) but the hierarchy and the magic number, six, remain. All of this would give us plenty to deal with by itself, but Pessen changes his ground, attacks definitions of class based on consciousness, and argues, "For most of American history I believe that wealth has been the single best clue to class position" (page 1294). It is hard to have it both ways.

The history of attempts to define and describe classes is riddled with failure. But some failures are different from others. Like the poor in nineteenth-century philanthropy, some are worthy, and some are not. In fact, everyone fails in some respect, but some, at least, climb right into the ring. Time after time, historians, sociologists, and other social scientists have tried to wrestle class to the ground. But, always, they never, even the best of them, quite succeed. If the arms are pinned, the legs are moving, one shoulder isn't quite flat, the opponent always bounces back. I know. I have tried often enough myself, and every time I do a little bit better. But class is the toughest, slipperiest opponent in the lexicon. Why is it so? The reason is

the same as the weakness in the wrestling metaphor. To talk of wrestling with class is to turn class into a thing. But class is a relationship, and relationships never can be grasped, held, and pinned. Whether they are between a man and a woman, a parent and a child, or between classes, relationships always are changing, forever in motion, getting better or worse, more or less intense, smoother or rougher. Relations are peculiarly difficult to discuss partly because they are always in flux and partly because they are not objects. They exist; their power often rules, charms, or destroys human lives. But they have no tangible existence by themselves. They cannot be seen, felt, or touched. As a result of those relations people often end up in groups or can be put in categories. But the groups or categories are not the same as the relations themselves.

Its quality as a relationship gives class both its elusive, maddening character for the social analyst and its power as a social force. For class refers to the enduring social relations that emerge from the way in which material life is produced. To use class as a concept that describes a relation is to think of societies as having motors, fundamental forces that drive, shape, and energize them. In this sense, class is a dynamic idea, an analytic tool, a force, always, for change. Professor Pessen ignores (or possibly, for some reason that he does not disclose, rejects) the notion of class as a relation. The use of class as a relation, however, has inspired much of the most creative, dynamic historical writing of the last several years, starting most notably with E. P. Thompson in England and spreading to America through the younger labor historians on whom he has had a profound influence.

To think of class as a relationship is to make a distinction between class and stratification. Professor Pessen mistakenly believes that adopting a perspective that views a social formation as dominated by two or three great classes closes down the possibility of fine-grained analyses of the most important distinctions between groups that might be thought to be part of the same classes. Not at all. The analysis of stratification is not the same as the analysis of class. People may be arrayed in hierarchies according to the possession of any attribute (wealth, income, prestige). These distinctions are important and consequential. They may or may not have a theoretical basis. They may or may not have some relation to class. But the analysis of class is not the same as the analysis of stratification, as even many American sociologists would agree. Indeed, the literature is studded with discussions of the distinction between the two concepts. Pessen, however, cheerfully ignores the difference between class and stratification. In this way, he misses the chance to use class as a powerful tool for social analysis or to explore the dynamics of social structure. All we are left is the conclusion that American social structure has been unequal and that its inequities reflect birth and privilege more than the natural ability of people. For this we have needed almost twenty years of the new social history.

ONE TEST OF A DEFINITION OR A CONCEPT is the kind of questions it provokes, the direction in which it points research. For Professor Pessen, "The great question concerns the proportions of the population enrolled in each of the classes over

time" (page 000). Why is this the great unknown? Is this simple numerical fact really the most important or interesting question about class for American historians? I hope not. In fact, I can think of a few others. None of them are original, and Pessen might have pulled a few of them, or others of equal interest, out of the great body of literature cited in his footnotes.

First, let's modify Professor Pessen's question. Not the proportion enrolled in each class over time (a curious metaphor, enrolled, as though class were a matter of choice; some sign up for the upper-upper, some for the lower-lower) but the origins and identity of classes over time would be a more useful way of putting the problem. That would mean finding the moments when modes of production began to change and identifying the new patterns of social relations that emerged. In American history it means looking much more carefully at the origins of capitalism and distinguishing between capitalism and industrialization for the prime features of capitalism—wage labor and the expropriation of surplus from producers—emerged, as Charles Tilly has forcefully stated recently, in agriculture and in cottage industry located outside of cities.⁴ Of course, social relations cannot be simply deduced from a general concept like capitalism. Their content and quality vary with circumstances. Contexts—culture, demography, politics, and so on—mediate the great forces of capitalism, making social structure and social relations particular and distinct.

The issue of context raises other questions. Culture is one of them (which Professor Pessen largely ignores). Have there been distinct class cultures in America? What has been their content? How have they been perpetuated, attenuated, or destroyed? Have they shaped important personal behavior such as fertility and child-rearing? Have they affected gender roles and family relations? Another is the nature of stratification within classes. Take some issues about the working class. How useful or accurate is it to talk about a labor aristocracy? This, of course, has been one of the great issues within British historical writing in recent years, and it has important implications for the American experience as well. Or, to take another question, should one think of the dependent poor as a lumpenproletariat outside the working class or as the most unfortunate members within it? What if dependence was a structural, predictable aspect of working-class life? Think, too, about the implications of dual-labor market theory. Can it be applied to the early nineteenth century? How does it affect the interpretation of group behavior, such as the activities of trade unions in the late nineteenth and early twentieth centuries?

Maybe the most important form of mobility for a working-class person in the late nineteenth and early twentieth centuries was out of the secondary and into the primary labor market. Thernstrom and his successors have shown that mobility between classes has been limited. This was important to know and now seems beyond question. But it does not end questions about mobility, because people clearly changed jobs a great deal and large and consequential differences obtained between the annual wages of workers. Quite elaborate hierarchies existed within individual firms, and workers in larger firms appear to have been paid better than

⁴ *Ibid.*, 179–89.

those in smaller ones of the same type. All of this means shifting the focus of mobility studies to the careers of people within the working class. Was it reasonable for workingmen to aspire to climb what John Foster has called the “limited ladders of mobility” within individual industries?⁵ Was it more reasonable for some workers rather than others to have those aspirations? Did the existence of those limited ladders keep open an illusion of opportunity and so dampen political unrest? We don’t know the answers to any of these questions, but all of them are crucial for understanding social structure and its connection with both the quality of individual life and the nature of political activity.

Not only the working class is problematic. Consider the middle class, as it is usually called. What a useful term. It translates a social relation that grows out of productive forces into a spatial metaphor. It is cleansed of any association with exploitation, any element of conflict. But who are the middle class, and is the term very useful? I have argued elsewhere that business class is a better way to encompass the second great class in the middle of the nineteenth century. Here let me pose the problem of what comes later—namely, interpreting the class position of the great army of white-collar workers and salaried professionals who first appear in the late nineteenth century. One way into the problem may be by thinking of the production of services as a new mode of production with its own set of social relations superimposed on an older one. Here, bureaucracy takes the place of the factory as the paradigmatic form of organization. Class structure, then, becomes a complex overlay of groups with differing social relations to each other—relations, that is, to those joined within the same and different modes of production. This formulation is crude and full of problems. But it is, at the least, a way of thinking about the problem that could prod research on class in a useful direction.

One final question about class in American history. How is one to interpret the relations between class and ethnicity? It is a truism to point out that America has been a multi-ethnic as well as a capitalist society. But to move beyond that point to a way of joining both in a satisfactory account of social development is very difficult. First, there is the matter of personal identity. Did people have dual identities; did they think of themselves sometimes as working class, sometimes as Irish or Italian? Or is identity more one dimensional? Were there great ethnic-class cultures that shaped personal and social relations throughout American history? The answer is not clear. On the side of ethnic-class identity is the organization of the working class around ethnic institutions. Until the 1920s, for instance, Catholic parishes and parochial schools most often were formed along ethnic lines. Within large industries, employers managed to manipulate ethnicity to keep the working class divided, and, it has often been argued, the hostility between ethnic groups helped defeat the possibility of a mass-based working-class movement in America. But there is another side, too. Ira Katznelson has recently argued persuasively that the politics of working people have been the politics of class at work and of

⁵ Foster, “Nineteenth-Century Towns: A Class Dimension,” in H. J. Dyos, ed., *The Study of Urban History* (London, 1968), 281–300.

neighborhood and ethnicity at home.⁶ For a variety of reasons, this pattern, which formed in cities in the mid-nineteenth century, has burrowed trenches that have channeled urban political life ever since. Convenient trenches, too, because the focus on neighborhood and ethnicity prevents the coalescence of working people around the real issues eroding the quality of urban life. From a slightly different direction, Stephen Steinberg recently has mounted an attack on historians and social scientists who stress the primacy of ethnicity over class throughout most of American history.⁷ I can't begin to argue the merits of these positions here. Rather, my point is simply to raise another crucial issue for historians of American social structure.

I HAVE MADE A LONG DIGRESSION to show that Edward Pessen's great question is not the great question at all. Unless one accepts his six classes, in fact, it isn't even valid.

MICHAEL B. KATZ

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⁶ Katznelson, *City Trenches: Urban Politics and the Patterning of Class in the United States* (New York, 1981).

⁷ Steinberg, *The Ethnic Myth: Race, Ethnicity, and Class in America* (New York, 1981).

Reply:

ALTHOUGH ROBERT H. WIEBE'S AND MICHAEL B. KATZ'S CRITIQUES of my essay occasionally overlap—particularly with regard to the intellectual sources of my rendering of the American class structure and my disagreement with the Marxian theory of class—I shall deal with them singly, in the order in which they appear.

PROFESSOR WIEBE ATTEMPTS at the outset to translate my essay for “perplexed readers,” so that they may understand what I *really* mean. That he finds in my discussion a “heavy camouflage of theory” and a “disguise” to be stripped away (page 1326) tells us more, I fear, about Wiebe’s own perplexing perception than about my argument. In choosing an “alternative conception” to the Marxist theory of class, I relied neither on W. Lloyd Warner’s nor on any other “anti-Marxist” theory but on what I and many others (sociologists and laity alike) have observed: that upper, middle, and lower classes could be found in most American communities and that, given the important internal differentiation within each of these classes, they could sensibly be further divided in two. My only textual reference to Warner is a criticism of his criteria for dividing the upper- from the lower-upper class. As my own criteria for class make clear, I have no use for Warner’s notion that the class into which individuals fall is determined by the subjective rankings given by some members of the community. (I have equally little regard for the “elitist school’s” method of locating power by eliciting the opinions of selected informants.) Wiebe’s unhappiness with the alleged melting away of my six-level class structure when I shift to an examination of politics suggests that he wants me to relate one political action to the interests of the lower-upper class, another to the interests of the lower-middle, another to this or that group. Forgive me for refusing to be so mechanical. The class structure was and is complex, and I try to describe this complexity. I see no need for continually referring to the fact of its complexity in my discussion of politics and class.

Professor Wiebe says I argue that a small upper class “determined the agenda for public policy” (page 1326). I neither wrote nor believe this. For, as I have observed, there has been an “absence of ideological cohesion among the highest social clusters” (page 1317). Nor do I quite say the words he puts in my mouth, that major party “politicians have assimilated the upper-class world view” (page 1326). It is not simply that I dislike that last, faddish phrase. What I do say is that major party politicians have been “sympathetic to the fundamental arrangements underlying” the class structure and have shown an “ideological affinity for the prevailing social order” (pages 1325, 1323). There is more to world view, however defined, than acceptance of a particular social structure. Wiebe asks, “What useful meaning can the ‘autonomy’ [that I attribute to the American political system] have when its directors reflect so consistently the interests of the upper class?” (page 1326). This is

a curious question, akin to asking, What useful meaning can freedom have when it so consistently does not lead to happiness? My answer is that I know not what precisely is the “useful” or any other meaning of the fact of autonomy. I only know it to be the fact and, I think, an interesting and important one. Among other things, the fact of autonomy leaves the future open.

In stating that I dismiss the “existence of an upper-class consciousness” (pages 1326–27), Professor Wiebe has misread my point that, in trying to determine the social positions of individuals of whatever class, their subjective estimates of their own class standing are often unreliable. The values or “consciousness” of individuals who happen to be in the same class are often dissimilar. In Wiebe’s view, to see life—or that aspect of it connecting the American class structure and politics—as I do, makes one an activist. Thus, according to him, I lament the “absence of truly liberalizing reforms” (page 1327). As historian, I lament nothing. Unlike Marx, I am content merely to understand the world. I can think of few ideas more discouraging to scholarship than Michael Katz’s evident belief that an appropriate test for social history is that it buttress “the movement for social justice” (page 1329). That is a political test. The only proper scholarly test for an idea is, How good is it?—that is, how intelligent, learned, original, clear, gracefully communicated, interesting, well founded, and wise?

Professor Wiebe faults me for denying an “affinity to Marxism” that ostensibly cuts me off from “theoretical resources” that, according to him, give “precisely the definitions and explanations” my essay needs (page 1327). Why, he asks, don’t I pay attention to Antonio Gramsci? I answer, What’s Gramsci to me, what am I to Gramsci? An implicit point of my essay is that it is not necessary to adhere to Marxism and the rigidities of its dialectic to understand that the times are usually out of joint, that society’s most envied rewards often go to the undeserving, that the politically powerful are often up to no good—certainly their demagogic pieties cannot be fully credited. While bored and depressed by the pretentious, dull, and unoriginal inanities of too many modern Marxists, I have learned a great deal indeed from that mighty romantic, Marx himself, as I have learned too, I trust, from Plato, More, Shakespeare, Cervantes, Hobbes, Godwin, Shelley, Dickens, Dostoyevsky, Balzac, Zola, Trollope, Thackeray, Austen, Meredith, Tolstoy, Chekhov, Beckett, Brecht, Fuentes, and American thinkers too numerous to mention.

I shall not take up Professor Wiebe’s exercise on the problems encountered in linking local to national class structures, except to note that it is based on a misreading that I have already discussed—my alleged dependence on the Warnerian method for locating the class identity of individuals and families. In passing, had Wiebe read more carefully my comparison of a single class structure to the “textbook manor” (page 1297), he would not have had to wonder whether “a national upper class might be a mere fiction to cover . . . innumerable local elites” (page 1327). I am not certain his comment that my “upper class lies frozen across the centuries” (page 1327), though nicely written, does full justice to (1) my fairly extended discussion of why the term, the American class structure, “is really a mere figure of speech” (pages 1296–97); (2) my view of how dissimilar local structures are to one another at any given time and how much they change over the course of

time; (3) my extended summary of the no less than sixteen historical developments that, I suggest, bear further study for the light they would throw on changes that have overtaken the American class structure (pages 1293–96); and (4) what I think is my clear explanation that I have “averaged out” across space and time what were actually diverse class structures primarily in order to make the discussion more manageable (page 1296). Further justifying my treatment are empirical data “that imbue the whole with a degree of homogeneity” (page 1297).

I am puzzled by Professor Wiebe’s reference to my alleged “easy dismissal of local governments as beneath the concern of the ruling class” (page 1328). I do not speak of or believe there was a “ruling class,” and neither here nor elsewhere do I express the view that the politically powerful regarded local government, even in Brooklyn, as “beneath the[ir] concern.” Elite indifference to holding local office did not signify indifference to how such office was used. In saying that I write “as if a commitment to property rights alone had constituted the culture of the ruling class” (page 1328), Wiebe distorts my argument that such a commitment constituted not the *culture* of the upper classes—a complex matter that would require much additional investigation—but the central idea in their thinking about the social order. As for Wiebe’s criticism that I “shelve” or set aside too quickly “the puzzling passivity of the ruled” (page 1328), I can only redirect him to my observation that this crucial matter, while indeed “outside the scope” of my essay, has been and continues to be the subject of a vast literature, to which I have made several contributions (pages 1318–19 note 65).

As a young historian not yet at the fourscore mark, let me assure Professor Wiebe that some day I hope to examine that “second quarter of the nineteenth century” (page 1328) that he finds so crucial and perhaps offer some observations on the ties between riches, class, and power in those local communities that he evidently thinks I have ignored. His concluding remark that I have left readers to fill in “many blanks” is doubtless true. Since I have tried to offer an explanation of the ties between American politics and class structure that is based on a biblical generation’s worth of research in and reading about, and more than a half-century of thought about, both themes, I trust that my “designs” for such an explanation may be found stimulating to that army of persons whose interest, like mine, is not in “class analysis” but in sensible analysis.

MICHAEL B. KATZ BEGINS BY REGALING US with an extended discussion of his fondness for books that, by puncturing myths, promote the “movement for social justice” (page 1329). Here I had thought that puncturing myths is achievement enough, no matter what the consequences—so long, that is, as the myths in question deserve to be deflated.

Professor Katz is inaccurate in a number of particulars. My essay is not precisely the “appeal for a closer relationship between social and political history” that he says it is (page 1329). The point I make is not that “any political historian would disagree” (page 1329) with the idea that explanations of political history are likely to be enriched by invoking class but rather that relatively few explanations of this sort have been attempted to date. I do allude to the growing number of such studies

(note 9 page 1292), but I remark, too, the absence of an overview for all of American history.

Professor Katz is unhappy with my implicit definition of politics, which he finds too narrow, and with the questions “usually asked by political historians,” which he finds old-fashioned (page 1329). “Even family life has its politics” (page 1330), he reminds us, and he discerns weighty political analysis in some of the new social histories that, of course, say nothing about politics as understood by us benighted antiquarians. The “backgrounds of office seekers” do not much interest Katz (page 1330), despite all the fascination the theme has had for several generations of political scientists and theorists, historians, and sociologists, who appear to have been under the impression that such investigations do seem likely to throw light on how “class relations” affect “various political arenas” (page 1330). It is charitable for Katz to “overlook” my “neglect” of social structure in Britain, a fascinating theme that, though not germane to the issues raised here, has attracted my attention in *Most Uncommon Jacksonians* (1967) and a number of other writings.

I am saddened by Professor Katz’s suggestion that I should have read Stephan Thernstrom’s *Poverty and Progress* with greater care, for I have read and thought well enough of that book to cite it at length in *Three Centuries of Social Mobility in America* (1974). What Katz means is that he finds no sign in my essay that I agree with what I have elsewhere described as Thernstrom’s “savaging” of Warner’s work on class. Not to agree with Katz’s perception of every point made in a book does not mean that one has read it carelessly. Katz seems unaware that long before 1964 “mainstream” sociologists took Warner to task for his reliance on subjective impressions.¹ More to the point, to divide the American social structure into six classes does not imply reliance either on Warner’s classification scheme or his method of arriving at it. Neither I nor the many sociologists who treat of six or five classes derive either these numbers or our reasons for using them from what Katz calls a “largely discredited formulation of [the] 1940s” (page 1331).

As I believe my discussion makes clear, I am quite aware of the complexities and dissimilarities in American class structures over time. I created a single “textbook structure” not because six is a magic number but because I thought it useful to think of a single or average class structure against which political events can be appraised. In trying to explain the “logic” of my supposed reliance on Warner, Professor Katz wrongly argues that I regard debates about the definition of class as “pointless” (page 1331). His assertion that I think *all* criteria should be included in a definition is similarly inaccurate. I *do* specify the reasons why I think social clusters change over time (pages 1294, 1296). I do *not* attack definitions of class based on consciousness but argue only that, in Harold Hodges’s language, “persons occupying a given [social] level need not be conscious of their class identity.”² That Katz thinks I have substituted a mere “list [of criteria] for a definition” of class (page 1331) puts him at odds, true, with one of the nation’s leading authorities on the subject, who “heartily approves the important theoretical points in Edward Pessen’s

¹ See Harold W. Pfautz and Otis D. Duncan, “A Critical Evaluation of Warner’s Work in Community Stratification,” *American Sociological Review*, 15 (1950): 205–15; Harold Hodges, *Social Stratification: Class in America* (Cambridge, Mass., 1964), 85; Leonard Riessman, *Class in American Society* (New York, 1959), 130–34; and Joseph A. Kahl, *The American Class Structure* (New York, 1951), 27–29.

² Hodges, *Social Stratification*, 13. And see page 1295, above.

multidimensional stratification structure,”³ but it is not surprising in view of Katz’s own belief that the “identity [of class] is determined by the underlying properties of a mode of production.”⁴

Katz appears to find contradictory my emphasis on the value of wealth as an indicator of class and my multidimensional model of class. But there is no contradiction in finding that one among the many components of a phenomenon is the single best clue to it. Class, Professor Katz advises us, is not a thing but “a relationship” (page 1332). He is not off by much, but he is off slightly, I fear. Classes contain individuals who are indubitably in relationship to one another and classes have a relationship to other phenomena. But a class of whatever sort is a thing or a group—albeit a slippery one. That classes, like E. H. Carr’s historical facts, are not dead fish lying on a slab but live fish swimming in the sea makes them neither water nor the relationship between what they are and the milieu they inhabit. I am quite aware that subtle distinctions can be found in the two classes that dominate the Marxist conception of the social structure. I am unhappy with Professor Katz’s own model not because its classes are few in number but because his scheme, among its several weaknesses, fails to recognize or deal with agriculture, lumps petty clerks with capitalists, and underestimates the great and significant disparities among individuals who might share a common “relationship” to the means of production.

In proclaiming that “the analysis of stratification is not the same as the analysis of class” (page 1332), Professor Katz contradicts the work of an entire generation of American sociologists. To say that “*even* many American sociologists would agree” (page 1332, italics added) seems somewhat patronizing. When he writes that “the literature is studded with discussions of the distinction between two concepts” (page 1332), he must mean the highly specialized, doctrinaire literature that he finds congenial. As his own bibliography and notes in recent writings show, he is indifferent to many other sociological writings that make no such distinction.⁵ Impressed by a recent book arguing that American sociologists retreated from class to stratification for ideological reasons, Katz seems unmindful of the point made by Bernard Barber that, whether you call them strata or classes and subdivide them into two or six or twelve groups “has nothing to do with the possibility of differences and conflicts of interest” among them.⁶ It is not what you call it that makes a rose smell sweet.

As for Professor Katz’s quandary over my “great question” (pages 1332; 1300), two points need to be made. When one hears that, let’s say, a new kind of social history is becoming more characteristic among historians, what could be more natural than to respond, The great question is, How characteristic is it? After summarizing a series of reforms that somewhat altered the shape of the social structure, I asked, The great question is, “How much have they altered it?” (page 1320). At least I have been consistent in my usage. My “great question” about the

³ Bernard Barber, Commentary on Pessen, “Toward a Clearer Overview of the Evolving American Class Structure,” session at the annual meeting of the Social Science History Association held in Nashville, Tenn., October 23, 1981 (unpublished typescript), pp. 2–3.

⁴ Michael B. Katz *et al.*, *The Social Organization of Early Industrial Capitalism* (Cambridge, Mass., 1982), 42.

⁵ *Ibid.*, and Katz, “Social Class in North American Urban History,” *Journal of Interdisciplinary History*, 11 (1981): 579–605.

⁶ Barber, Commentary on Pessen, pp. 4–5.

numbers “enrolled in”—or found in, identified with, affiliated with, having membership in, belonging to—the different social classes followed my description of the drastically dissimilar characteristics of these classes. It is the “great question” only in that context. (For the truly Great Questions we must turn to Tin Pan Alley, which has given us, “Why Was I Born?”, “Who Cares?”, “What Is This Thing Called Love?”, and, more recently, “What’s It All About, Alfie?” Alfie, need I say, is Everyman.) Katz’s puzzled reaction to my question about the proportions of population typically occupying one or another of the social levels is itself puzzling.

As his research makes clear, Katz is a quantifier whose work rests heavily on numbers. One out of every five pages of his two most recent books is devoted to tables, equations, numbers; and his prose is heavily sprinkled with mathematical statements. It is strange that when numbers are very important—as clearly they are in trying to understand the shape of the American class system—he becomes oblivious to their charms. To most people, even of a nonmathematical cast of mind, it is a great distinction indeed that the lower social levels, whether called “working class” or “lower strata,” are inhabited by 10 rather than 30 percent. So likewise would be the distinction between an upper or capitalist class that contained less than 1 percent of the population rather than 33.3 percent (the proportion that Katz and his colleagues have recently located in what they call the “business class” of the nineteenth-century Canadian lakeport town of Hamilton).

I believe that most readers will find in my essay an awareness that social classes “vary with circumstances” (pages 1333; 1297–98). In contrast to Professor Katz, I do not see changing contexts “mediat[ing] the great forces of capitalism.” Unlike many new social historians, who seem most ecstatic when they find something “mediating” among or between the most unlikely other things, I am rarely aware of mediation outside of labor-management negotiations. I may well have ignored “culture” (at least as Katz defines it), but I do think that most readers will discern my awareness of the effect of class on “important personal behavior such as fertility and child-rearing” (page 1333; 1293–96).

Professor Katz is confident that thinking about class *his way*—however “crude and full of problems” it may be—“could prod research on class in a useful direction” (page 1334). Perhaps so. Like the priest in the great postwar Italian film *Open City*, I believe there are many paths to heaven. In the meantime, I draw some comfort from the public assessment by a stranger to me personally, the eminent Bernard Barber, that “all specialists in the field of stratification are in debt to Professor Pessen for his excellent and original synthesis . . . , a gift to all Americanists [that provides] the base from which good work in the social history of American class can go forward.”⁷ As an inveterate skeptic, I am uneasy about subscribing to such lavish encomiums. Yet I cannot help being impressed by the discernment and, more seriously, the authority of the scholar who offered them.

May the discussion indeed go forward.

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⁷ *Ibid.*, pp. 2, 8.

Research Note
Pearl Harbor, Microdots, and J. Edgar Hoover

JOHN F. BRATZEL
and
LESLIE B. ROUT, JR.

SINCE THE DAY ITSELF, Americans have never ceased to wonder why and whether the Japanese attack on Pearl Harbor, December 7, 1941, could have been such a surprise. Recently, John Toland has revived the issue by asserting that President Franklin D. Roosevelt must have known the attack was impending from various sources, including intercepted radio transmissions from the Japanese carrier fleet as it sailed toward the Hawaiian Islands.¹ But Japanese officers aboard that fleet—including Captain Minoru Genda, who helped plan the assault—have asserted that Toland is in error on this point: radio silence, they maintain, was never broken during the voyage.² Doubtless, Toland's evidence will long be discussed and disputed. No attempt can be made here to verify it or the conclusions he drew from it. Instead, this essay is concerned with important evidence concerning Japanese intentions that reached British counterintelligence and the Federal Bureau of Investigation months before the attack, evidence that Toland says he sought but did not find.³ Toland concluded, furthermore, that the alleged "disappearance" of this evidence, along with other documents, could not be a "coincidence" but was part of a "cover up" intended to purge intelligence records damaging to high officers and officials of the United States, who preferred to put the blame for Pearl Harbor upon the two commanders in Hawaii, General Walter Short and Admiral Husband E. Kimmel, rather than bear it themselves.⁴

Research Note is a new feature of the *AHR*. In it, the editors expect to publish short articles on recent archival discoveries or other revelations of major historical significance. We invite the submission of appropriate manuscripts. On acceptance, the article will be published in the next available issue. Prospective authors should write the editors for a copy of the "AHR Guidelines for Authors." THE EDITOR

¹ Toland, *Infamy: Pearl Harbor and Its Aftermath* (New York, 1982), 249–324.

² "Historian Says Navy Detected Japanese Move toward Pearl Harbor," *New York Times*, March 13, 1982, p. 4.

³ "There are no available records in the F.B.I. concerning Popov's questionnaire. Hoover's second in command, Edward Tamm, never heard of it but if Hoover had received the information, he told the author, he would certainly have passed it on to Roosevelt." Toland, *Infamy*, 260 n. 4.

⁴ Toland, *Infamy*, 322 n. 5.

The “missing” evidence consists of information on what steps J. Edgar Hoover and his aides at the Federal Bureau of Investigation took, or did not take, to inform the White House, the Military Intelligence Division (MID), and the Office of Naval Intelligence (ONI) concerning an espionage mission to Hawaii ordered by the German Secret Service obviously at the behest of the Japanese government months before the attack. Although the spy, who was a double agent, revealed his oral and written (a questionnaire) instructions to the FBI on landing in New York, documents in the files of the FBI and the Roosevelt Library at Hyde Park show that Hoover sent only a harmless part of the questionnaire to the president. These documents do not tell us conclusively what Roosevelt knew and when he knew it. But they do show what he was not told by the FBI, and they raise another question about the competence of Hoover and his associates in evaluating foreign intelligence during World War II.

THE STORY BEGINS IN YUGOSLAVIA in 1939. Dusko Popov, a sometime lawyer, sometime business promoter, and always the playboy, was approached by a German friend, Johann Jebesen. Jebesen had joined the *Abwehr* with the idea of avoiding front-line duty, but this organization would only tolerate him if he obtained useful information. Would his old pal, with whom he had attended school in Germany, go to France and report on which political figures would be predisposed to collaborate with the Nazis? The request made sense because Popov was a member of a wealthy family with far-flung business connections and possessed entrees into the highest levels of Parisian cafe society. After some thought, Popov agreed to do a favor for an old friend. Shortly thereafter, Popov gave his report to the *Abwehr*—but he also handed a copy to the British. This detailed statement concerning prospective sympathizers confirmed what the Germans already believed and thus established Popov’s credibility. It is not surprising, then, that the Germans asked him to spy for them in England. Once again, he agreed to aid the Germans (code name: Ivan)—but also the British (code name: Tricycle). Popov had become a double agent.⁵

Until the summer of 1941, Popov furnished Germany with data supplied by the MI-6 division of British intelligence. Since the Germans could verify much of his information from other sources, they grew to trust him implicitly and soon changed his assignment to the United States. Before he left, the Germans gave Popov, while on a trip to Lisbon, a list of questions in microdot form (see Appendix). This technological advance in microphotography was owed to a process—developed by a German professor, Arnold Zapp—that could reduce a written page to the size of the dot on an “i.” The spy could retrieve the information by using a microscope.⁶ Popov’s microdots were glued to a telegram blank, where they look like tiny smudges or paper defects (see figure 1).

⁵ Popov, *Spy/Counterspy* (New York, 1974), 17–20.

⁶ David Kahn, *The Codebreakers: The Story of Secret Writing* (New York, 1967), 525–26. The FBI had been warned in January 1940 to “watch out for dots—lots of little dots,” but did not obtain an actual microdot until August 1941. At that time, one was given to the FBI by Popov. The discovery was not owed to the genius of an FBI laboratory technician, as the FBI chief claimed after the war: Hoover, “The Enemy’s Masterpiece of Espionage,” *Reader’s Digest*, April 1946, pp. 1–6; and Popov, *Spy/Counterspy*, 200–04.

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<p>Nome e domicílio do expedidor: Popov Rua Bartolomeu Dias 120 Lisboa</p> <p>Hora a que o telegrama foi depositado (e):</p> <p>(e) No próprio interesse do público é conveniente indicar por extenso a hora a que o telegrama for depositado na estação, servindo de regulador o relógio da mesma estação.</p>			

Figure 1: Telegram containing the microdots, encircled and marked numbers 1 and 2 (with duplicates). Photograph of the telegram was one of the attachments to a letter of September 3, 1941, from J. Edgar Hoover to General Edwin M. Watson, presidential aide (see figure 2, below). Reproduced courtesy of the Franklin Delano Roosevelt Library, Hyde Park, New York.

Popov dutifully reported to the Lisbon agent of MI-6 that he had been instructed to organize a new spy network in the United States and seek answers to the questionnaire.⁷ Much of that document dealt with general matters, such as shipping, production, troop movements, armaments, and military organization. But fully one-third of the document asked specific questions about army bases and airfields on the island of Oahu and defenses of Pearl Harbor. The Germans wanted sketches showing the exact locations of installations at "Wicham" (Hickam), Wheeler, and "Kaneche" (Kaneohe) airfields. They likewise wanted sketches of the installations at Pearl Harbor and detailed information concerning dredging, depth

⁷ Popov, *Spy/Counterspy*, 149; and J. C. Masterman, *The Double-Cross System in the War of 1939 to 1945* (New Haven, 1972), 79–80.

of water, torpedo nets, anchorages, and the like (see Appendix). To obtain this information, the *Abwehr* directed Popov to visit Hawaii.

Although not yet officially at war, by mid-1941 the United States was actively aiding Great Britain by escorting convoys almost halfway across the Atlantic and by supplying arms through Lend-Lease. Moreover, the military and naval staffs of both countries were engaged in joint planning for U.S. entrance into the war. All this would seem to suggest that the British should have sent the questionnaire to Washington without hesitation. This was not the case. In fact, the FBI was virtually at war with MI-6.⁸ Some of the problem stemmed from U.S. inexperience in espionage and some from a lack of forthrightness on the part of the British. But the bulk of the blame must be placed on the shoulders of J. Edgar Hoover, who was so intent on monopolizing intelligence operations that he brooked no interference from the British. Unwilling to risk a snub, and convinced that Hoover would not believe them anyway, MI-6 told Hoover that Popov was coming, but little else. British intelligence assumed that the FBI would comprehend the significance of the inquiries on the questionnaire and act accordingly.⁹

As expected, the Yugoslav was met by FBI agents in New York. Later, Popov claimed that one of his first statements to New York Bureau Chief John Foxworth was, "You can expect an attack on Pearl Harbor before the end of the year. . . ." As proof, he was able to cite two sources: his friend Jebson and Baron Gronau, German air attaché in Tokyo, both of whom had recently escorted Japanese naval officials to Taranto, Italy, the scene of a devastating air attack against the Italian fleet by torpedo planes from the aircraft carrier HMS *Illustrious* in November 1940. The Japanese had wanted to know all about the attack in infinite detail, and both Gronau and Jebson had concluded that the Asian member of the Tripartite Alliance was planning to duplicate the British feat.¹⁰

This anecdote was not in itself conclusive. Officials are always predicting dire events, and the curiosity of the Japanese concerning a successful surprise attack was natural. The prediction of Gronau, who was not a secret agent, came indirectly through Jebson and, hence, was perhaps suspect. But Popov did have hard evidence—the microdot questionnaire. Here was tangible proof of Japanese planning. How could it be ignored? It was, and so was Popov. He was told to wait two weeks to see Hoover. In the interim, he went to Florida with a girl friend, an idyll that was rudely interrupted when the FBI threatened him with prosecution for violating the Mann Act. When they finally met, the FBI chief chastised the Yugoslav for his immoral ways and said, according to Popov, "You're begging for information to sell to your German friends so you can make a lot of money and be a playboy." Denied the trip to Hawaii, Popov remained in New York for a few months, until the FBI allowed him to go to Rio de Janeiro. There he was to make

⁸ See William Stevenson, *A Man Called Intrepid* (New York, 1976), 160–65; National Archives and Record Service, Record Group 59, 841.01B11/226, August 5, 1941, pp. 1–2; and *ibid.*, 841.01B11/191, February 5, 1941, pp. 1–5.

⁹ Masterman, *The Double-Cross System*, 80.

¹⁰ Popov, *Spy/Counterspy*, 141–44, 158–59.

JOHN EDGAR HOOVER
DIRECTOR



Federal Bureau of Investigation
United States Department of Justice
Washington, D. C.
September 3, 1941

~~STRICTLY CONFIDENTIAL~~

Major General Edwin M. Watson
Secretary to the President
The White House
Washington, D. C.

DECLASSIFIED

E.O. 11652, Sec. 5(E)(2)
Justice Dept. letter, 9-21-72
By DBS, NLR, Date JUL 8 1975

Dear General Watson:

I thought the President and you might be interested in the attached photographs which show one of the methods used by the German espionage system in transmitting messages to its agents.

There is attached a photograph of a message written on a form of the Eastern Telegraph Company Limited which has two microphotographs placed on the left side and which are marked 1 and 2. These microphotographs are on photographic film which is pasted on an otherwise innocent document in such a way as not to be discernible upon casual glance at the document.

Each microphotograph contains two small dots marked 1 and 2 on the telegram form, each of which is made up of a paragraph of written material. The Technical Laboratory of the Bureau by magnifying this material four hundred times was able to develop its contents. I am transmitting herewith enlargements of these microphotographs, showing the printed material contained in them in German. There is also attached a translation of this material in English. You will note that the two microphotographs contain the same substance.

The microphotographs referred to above were secured in connection with a current investigation being made by the FBI.

With assurances of my highest regards,

Sincerely,

J. Edgar Hoover

Enclosure

Figure 2: Hoover to Watson, September 3, 1941. Hoover's covering letter about the microdots containing German intelligence directives to agents in the United States. Reproduced courtesy of the FDR Library, Hyde Park.

contact with the *Abwehr* network in Brazil, obtain technical information on radio use, and then set up a transmitter in New York. It was hoped that undercover German agents would contact Popov to use the radio, and, when that happened, the FBI expected to be waiting.¹¹

WHILE POPOV'S SUBSEQUENT CAREER IS INTERESTING, crucial questions remain: What happened to the questionnaire? Who got it? Who evaluated it? What did the intelligence services know? What did FDR know?

Apparently, only a small portion of the microdot material was seen by any American official other than Hoover and his aides. On September 3, 1941, Hoover sent to the White House a letter addressed to Brigadier General Edwin M. Watson, presidential secretary (see figure 2). This document, now in the Roosevelt Library at Hyde Park, notes that "the President and you might be interested in the photographs which show one of the methods used by the German espionage system in transmitting messages to its agents." It goes on to describe the microdot system, but, instead of discussing the substance of the microdot message, the letter ends by simply stating that the information was "secured in connection with a current investigation being made by the FBI."¹² Hoover used the information to demonstrate how efficient the FBI was rather than to warn of a possible attack.

Notwithstanding Hoover's puffery, the contents of the questionnaire still should have caught the president's attention, had he read it. Incredibly, he never had a chance. Hoover chose to edit the questionnaire, so that only about one quarter of the questions were included, none of which dealt with Hawaii (see figure 3). The president was sent a curiosity piece and probably read it that way.¹³

Even so, Hoover had the responsibility of sending this material on to the MID and ONI, which might well have better appreciated its importance. Yet the FBI now states that the only information sent to the army and navy intelligence offices on this occasion was a duplicate of that sent to the president.¹⁴ Neither the Naval Historical Center nor the military records section of the National Archives has been able to locate the letter of September 3 with its edited questionnaire.¹⁵ The full text of Popov's questionnaire still rests in the files of the FBI, where it has been for over forty years.¹⁶

The question begging to be answered is why. Why did Hoover not send the full text of what was in the microdots to FDR and other U.S. intelligence agencies? The answer seems to have its roots in the high-stakes battle to control U.S. intelligence. Hoover wanted to look good to the president and gain points against his rivals—

¹¹ *Ibid.*, 166–80.

¹² Hoover to Watson, September 3, 1941, Franklin D. Roosevelt Library, Franklin D. Roosevelt Papers, OF 10B, box 28.

¹³ That Roosevelt actually received Hoover's letter to Watson is evident from a "Memorandum for the President," signed "E.M.W." and dated September 4, 1941: "I thought you might be interested in this report on German methods of distributing information, just forwarded from J. Edgar Hoover." Roosevelt Library, Roosevelt Papers, OF 10B, box 28.

¹⁴ Roger S. Young, Assistant Director in Charge, Office of Congressional and Public Affairs, Federal Bureau of Investigation, to the authors, November 13, 1981.

¹⁵ Maida H. Loescher, Military Archives Division, National Archives, Washington, D.C., to the authors, November 18, 1981.

¹⁶ Roger S. Young, Federal Bureau of Investigation, to the authors, October 15, 1981.

TRANSLATION OF MICROPHOTOGRAPHIC MATERIAL

1.

All information regarding the American air-defense is of great importance. The answering of the following questions is urgent.

- I. How large is a. the total monthly production of airplanes?
b. the monthly production of fighter-planes?
c. the monthly production of pursuit-planes?
d. the monthly production of training-planes?
e. the monthly production of civilian-planes?
- II. How many and which of these airplanes were delivered to the British Empire, namely
a. to Great Britain?
b. to Canada?
c. to Africa?
d. to the Near East?
e. to the Far East and Australia?
- III. How many USA pilots complete their training each month?
- IV. How many USA pilots are joining the RAF?

2.

Reports regarding the Canadian Air-force is of great value. All news of number and type of the front-airplanes, number, location and numbers of squadrons is of interest. It is of special importance to learn in detail of the air-training plan being followed in Canada, namely the type, location and capacity of the individual schools, and if possible, also their numbers. According to the reports in question, each type of school (beginner-, advanced-, and Observer-) is numbered from 1 on.

Figures 3a and 3b: The microdot material Hoover sent to Watson: the German original of the last part of the questionnaire and the FBI translation of the same passages. (The FBI translation differs slightly from that prepared by British intelligence; see the appendix, page 1349, opposite.) Reproduced courtesy of the FDR Library, Hyde Park.

Alle Angaben über die amerikanische Luftaufklärung sind von großer Wichtigkeit. Die Beantwortung nachstehender Fragen ist vornehmlich.

- I.) Wie groß ist a) die Gesamtmonatsproduktion an Flugzeugen?
b) die Monatsproduktion an Kampfflugzeugen?
c) " " " Jagdflugzeugen?
d) " " " Schulflugzeugen?
e) " " " Zivilflugzeugen?

II.) Wieviel und welche Flugzeuge wurden davon an das Britische Empire geliefert und zwar

- A) an Großbritannien?
- B) " Canada?
- C) " Afrika?
- D) " Japan Osten?
- E) " Fernen Osten und Australien?

III.) Wieviel USA-Piloten beenden monatlich ihre Ausbildung?

IV.) Wieviel USA-Piloten treten in die RAF ein?

Meldungen über die kanadische Luftwaffe sind von großer Wert. Alle Nachrichten über Anzahl und Muster der Frontflugzeuge, Anzahl, Nummer und Standort der Staffeln interessieren. Von besonderer Wichtigkeit ist, Näheres über den in Kanada laufenden Luftausbildungsplan zu erfahren und zwar der Art, Standort und Kapazität der einzelnen Schulen und möglichst auch ihre Nummern. Nach vorliegenden Meldungen ist jede Schule (Anfänger-, Fortgeschrittenen- und Beobachter-) von 1 an nummeriert.

namely, the other U.S. intelligence agencies and MI-6. Clearly he found Popov and his style of living abhorrent and did not trust him. As a double agent who had already double-crossed the Germans, Popov was hardly above suspicion. Intercepted messages show that Popov, while in Brazil, received \$18,000 from the *Abwehr*, which he may not have reported to the FBI on his return to the United States and did not mention in his memoirs.¹⁷ In that book, he claimed to have given previously unknown details concerning the *Abwehr* network in Brazil to FBI agents in Rio de Janeiro. But apparently Popov supplied nothing.¹⁸

Regardless of Hoover's assessment of Popov's character, the director's failure to transmit the entire Popov questionnaire to the White House and to military and naval intelligence agencies shows both a poverty of judgment on his part and the crippling consequences of rivalry among those governmental agencies charged with gathering and evaluating information essential to the defense of the United States at a critical time in its history.

APPENDIX

The *Abwehr* Questionnaire*

Naval Information.—Reports on enemy shipments (material foodstuffs—combination of convoys, if possible with names of ships and speeds).

Assembly of troops for oversea transport in U.S.A. and Canada. Strength—number of ships—ports of assembly—reports on ship building (naval and merchant ships)—wharves (dockyards)—state and private owned wharves—new works—list of ships being built or resp. having been ordered—times of building.

Reports regarding U.S.A. strong points of all descriptions especially in Florida—organisation of strong points for fast boats (E-boats) and their depot ships—coastal defence—organisation districts.

Hawaii.—Ammunition dumps and mine depots.

1. Details about naval ammunition and mine depot on the Isle of Kukshua (Pearl Harbour). If possible sketch.
2. Naval ammunition depot Lualuelei. Exact position? Is there a railway line (junction)?
3. The total ammunition reserve of the army is supposed to be in the rock of the Crater Aliamanu. Position?
4. Is the Crater Punchbowl (Honolulu) being used as ammunition dump? If not, are there other military works?

¹⁷ National Archives and Record Service, Record Group 59, 862.20210 ENGELS ALBRECHT GUSTAV/71, Section 13, unpaginated.

¹⁸ Jack West, director of the FBI section in Brazil, 1941–43, to the authors, October 16, 1981.

Aerodromes.

1. *Aerodrome Lukefield.*—Details (sketch if possible) regarding the situation of the hangers (number?), workshops, bomb depots, and petrol depots. Are there underground petrol installations?—Exact position of the seaplane station? Occupation?

2. *Naval air arm strong point Kaneche.*—Exact report regarding position, number of hangers, depots, and workshops (sketch). Occupation?

3. *Army aerodromes Wicham Field and Wheeler Field.*—Exact position? Reports regarding number of hangers, depots and workshops. Underground installations? (Sketch.)

4. *Rodger's Airport.*—In case of war, will this place be taken over by the army or the navy? What preparations have been made? Number of hangers? Are there landing possibilities for seaplanes?

5. *Airport of the Panamerican Airways.*—Exact position? (If possible sketch.) Is this airport possibly identical with Rodger's Airport or a part thereof? (A wireless station of the Panamerican Airways is on the Peninsula Mohapuu.)

Naval Strong Point Pearl Harbour.

1. Exact details and sketch about the situation of the state wharf, of the pier installations, workshops, petrol installations, situations of dry dock No. 1 and of the new dry dock which is being built.

2. Details about the submarine station (plan of situation). What land installations are in existence?

3. Where is the station for mine search formations [Minensuchverbaende]? How far has the dredger work progressed at the entrance and in the east and southeast lock? Depths of water?

4. Number of anchorages [Liegeplaetze]?

5. Is there a floating dock in Pearl Harbour or is the transfer of such a dock to this place intended?

Special tasks.—Reports about torpedo protection nets newly introduced in the British and U.S.A. navy. How far are they already in existence in the merchant and naval fleet? Use during voyage? Average speed reduction when in use. Details of construction and others.

1. Urgently required are exact details about the armoured strengths of American armoured cars, especially of the types which have lately been delivered from the U.S.A. to the Middle East. Also all other reports on armoured cars and the composition of armoured (tank) formations are of greatest interest.

2. Required are the Tables of Organisation (TO) of the American infantry divisions and their individual units (infantry regiments, artillery "Abteilung," and so forth) as well as of the American armoured divisions and their individual units (armoured tank regiments, reconnaissance section, and so forth). These TO are lists showing strength, which are published by the American War Department and are of a confidential nature.

3. How is the new light armoured car (tank)? Which type is going to be introduced? Weight? Armament? Armour?

1. Position of British participations and credits in U.S.A. in June 1940. What are England's payment obligations from orders since the coming into force of the Lend Lease Bill? What payments has England made to U.S.A. since the outbreak of war for goods

supplied, for establishment of works, for the production of war material, and for the building of new or for the enlargement of existing wharves?

2. Amount of state expenditure in the budget years 1939/40, 1940/41, 1941/42, 1942/43 altogether and in particular for the army and the rearmament.

3. Financing of the armament programme of the U.S.A. through taxes, loans and tax credit coupons. Participation of the Refico and the companies founded by it (Metal Reserve Corp., Rubber Reserve Corp., Defence Plant Corp., Defence Supplies Corp., Defence Housing Corp.) in the financing of the rearmament.

4. Increase of state debt and possibilities to cover this debt.

All reports on American air rearmament are of greatest importance. The answers to the following questions are of special urgency:

I. How large is—

- (a) the total monthly production of aeroplanes?
- (b) the monthly production of bombers [Kampfflugzeuge]?
- (c) " " " " fighter planes?
- (d) " " " " training planes? [Schulflugzeuge]?
- (e) " " " " civil aeroplanes [Zivilflugzeuge]?

II. How many and which of these aeroplanes were supplied to the British Empire, that is to say—

- (a) to Great Britain?
- (b) to Canada?
- (c) to Africa?
- (d) to the Near East?
- (e) to the Far East and Australia?

III. How many U.S.A. pilots finish their training monthly?

IV. How many U.S.A. pilots are entering the R.A.F.?

Reports on Canadian Air Force are of great value. All information about number and type (pattern) of front aeroplanes [Frontflugzeuge]. Quantity, numbers and position of the echellons [Staffeln] are of great interest. Of special importance is to get details about the current air training plan in Canada, that is to say: place and capacity of individual schools and if possible also their numbers. According to reports every type of school (beginners'—advanced—and observer school) is numbered, beginning with 1.

*The full text of the *Abwehr* questionnaire, as translated by the British secret service, has been previously published in J. C. Masterman, *The Double-Cross System in the War of 1939 to 1945* (New Haven, 1972), 196–98; and Gustav Popov, *Spy/Counterspy* (New York, 1974), following 148. John Toland published that part of the questionnaire dealing with Hawaii, not the "complete German questionnaire," as he claimed; Toland, *Infamy: Pearl Harbor and Its Aftermath* (New York, 1982), 349–50.

Reviews of Books

GENERAL

FRANZ BORKENAU. *End and Beginning: On the Generations of Cultures and the Origins of the West*. Edited by RICHARD LOWENTHAL. (European Perspectives.) New York: Columbia University Press. 1981. Pp. ix, 493. \$24.95.

Scholarly etiquette is contemptuous of naiveté; among the many lessons academic training teaches, few are more eagerly learned than the techniques for avoiding self-exposure. In most of their books most professional historians eschew grand questions about the direction of history and about life's ultimate meanings—so much so that these posthumously collected essays by Franz Borkenau contain a kind of shock. They are remarkably consistent as they range from the birth of civilization to international affairs in the 1950s, for every page belongs—in its philosophic earnestness, its confident erudition, its personal engagement—to the intellectual world of Central Europe in an earlier generation.

What American professor of history writing in the 1980s would give the theories of Oswald Spengler a central place in current understanding of the past or could image claiming that “the theory of cultural cycles he created now dominates our historical thinking”? Borkenau criticizes Spengler for his emphasis on symbols and criticizes Arnold Toynbee even more sharply for his moralizing and his inadequate definitions of what distinguishes one civilization from another. But like them he sees civilizations as entities whose histories reveal fundamental laws of life. The five essays that constitute the first section of this book set a tone for the whole of it: in the course of discussing cultural cycles, they include moving passages on mortality, a striking reinterpretation of the Oedipus myth, and interesting asides on the relevant theories of Frazer, Freud, and others. Even in these richly informed speculations on history's largest patterns one begins to feel that the galvanizing poles of Borkenau's thought are the dark gloom of modern totalitarianism and the bitter

irony that civilization receives its creative spark from barbarian turmoil.

Most of the book consists of the twelve somewhat overlapping essays of the second section, essays that search for the sources of Western civilization and explore the culture of the early Middle Ages. Throughout, the focus is on the contributions of the barbaric northern or Germanic cultures. Their vitality, it is argued, combined with less clearly specified qualities absorbed from the classical world to form a new civilization. A brilliant essay on the use of the first-person pronoun (beginning with a runic inscription), essays on Germanic myth (with myth itself seen as the product of the specific historical stage at which a superior culture in decline and a rising more primitive one meet and clash), and several essays on early Christianity all display magnificent erudition and admirable sensitivity. Here traces of Borkenau's Catholic upbringing and of his experience with Marxist social analysis interweave in a rich historical vision whose Hegelian and philological coordinates function as a framework to tie together everything that the cultural sciences can teach. Byzantine and Christian art, like poetry and language and archaeology, are all material for independent and even daring interpretation in essays that can excite the old-fashioned intellectual in all of us—and that seem certain to offend the modern specialist at point after point.

The feeling for apocalypse, an attraction for its terrible beauty and its ultimate seriousness, is never far from Borkenau's vision. And it becomes more explicit in the three essays of the final section, essays about Jews (and Toynbee), the Cold War, and the possibility of nuclear holocaust. Borkenau's earnestness leads to Manichean anger that threatens in these essays on contemporary topics to overpower his gentler wisdom. His was a *Weltanschauung* that left little room for the indifference of moderation, the optimism of pragmatic approaches, or mere bourgeois balance. Seeing his civilization threatened by an imminent end, it took all his intellectual strength to imagine the distant beginnings that can

once again emerge from barbarism. For Borkenau the naïveté to be avoided was hope; his old-world quest for a kind of universal knowledge proved its scientific respectability by insisting that this civilization was doomed. The intellectual heroism of Borkenau's effort, even more than the provocative intelligence with which he went about it, justifies Richard Lowenthal's careful labor in assembling and introducing these provocative, troubled, and dated essays.

RAYMOND GREW
University of Michigan,
Ann Arbor

RICHARD S. WESTFALL. *Never at Rest: A Biography of Isaac Newton*. New York: Cambridge University Press. 1980. Pp. xviii, 908. \$49.50.

Richard S. Westfall is a leading Newton scholar and the author of a number of articles concerning various aspects of Newton's life and career as well as an important monograph on the concept of force in Newton's physical science. The present work, based on extensive research among manuscript and primary printed sources, deals in great detail with every aspect of Newton's life and times.

Here are displayed the many varied aspects of Newton's activities, from his studies of theology and explorations of alchemy to his final years in London directing the operations of the mint during the recoinage and ruling the Royal Society with an iron hand. Of course, what makes Newton of interest is not the set of activities that fill the greater part of this biography, but rather the important contributions he made to the fields of mathematics, dynamics and celestial mechanics, and optics. Newton's activities and achievements in these three areas are explored here in relation to the general background of the times, so that the untutored reader may gain some understanding of the magisterial achievements of Newton in bringing to their first full climax the efforts of those men of genius who had set going the Scientific Revolution.

It is a matter of note, however, that despite the thousands of pages of manuscripts of Newton, there is a considerable area of uncertainty concerning the exact stages of the development of Newton's scientific thought. Accordingly, it must be put on record that many of the crucial steps in the reconstructions of the development of Newton's scientific ideas are presented here from a point of view that is not shared by all other scholars in the field. This statement is not intended to be a direct criticism of Westfall so much as a description of the state of our knowledge—a similar comment would be made no matter who had written the biography.

For example, it is now well known that Newton

did not really understand how to analyze orbital motion into a linear inertial component along the tangent to the curve and an accelerated component directed toward the center until he corresponded with Hooke on this topic in 1679–80. Newton then solved the problem of the kind of force directed toward the sun that keeps a planet in an elliptical orbit. Most scholars, myself included, assume that the proof that Newton then worked out was more or less the one he presented in his eventual *Principles*, or in some preliminary tracts to that great treatise. Westfall, however, assumes that Newton's proof was quite different, namely, that contained in a paper Newton wrote up (presumably) and sent to the philosopher John Locke after the *Principles* had been published. There are also important differences of opinion with respect to the analysis of some of Newton's fundamental concepts in dynamics, in particular the role of alchemical ideas in Newton's more orthodox scientific thought, as in dynamics and celestial mechanics.

One of the great achievements of the book is to make clear to any reader the degree of Newton's involvement in alchemical studies. There is also much new information concerning Newton's explorations of theology. Finally, one should note the impressive new documentary materials concerning Newton's activities in London in the late years of his life, chiefly in relation to the mint. This is a very large book, but considering the kind of documentation and the variety of Newton's interests and activities, it could easily have been longer. I believe it fair to say that there is much new material in it even for Newton specialists. Although there have been other biographies of Newton in the past, this is the first one to be based on an extensive use of the manuscript sources. It is more than merely the biography of a man and an account of the development of his ideas. It is a fundamental resource work on the Scientific Revolution, a work that should be on the shelves of every scholar interested in the main currents of ideas of the seventeenth century or any aspects of the early history of modern science.

I. BERNARD COHEN
Harvard University

MARION L. KUNTZ. *Guillaume Postel, Prophet of the Restitution of All Things: His Life and Thought*. (International Archives of the History of Ideas, number 98.) Boston: Martinus Nijhoff. 1981. Pp. xv, 270.

Guillaume Postel, for his extraordinary breadth of learning, his considerable intelligence, and his eccentric ideas, exercised a fascination on his own contemporaries throughout sixteenth-century Europe and has much to tell us now about the variety of thought that flourished during that period. He

sought to find a unity behind all cultures that would provide the basis for effective missionary activity, devised a new religious formulation based on the transformation of sexuality, preached a coming new age, and proclaimed visions that cause the author of this biography to wonder about mind-altering substances.

The last monograph in English to treat Postel is William Bouwsma's *Concordia Mundi*, which appeared in 1957 and focused on the context and meaning of his many writings. More recently François Secret has published additional manuscripts of Postel and explored his relation to esoteric literature. In the present study Marion L. Kuntz is primarily concerned with Postel's biography. Making use of the additional published manuscripts as well as her own work, largely in the Venetian archives, the author claims to present the facts of Postel's life as accurately as possible.

Certain aspects of Postel's life emerge much more clearly and fully here than they have in previous scholarship, especially his years in Venice. The author has convincingly identified the Venetian woman who inspired Postel and whose visions and pious life led him to formulate his new conceptions of religion. The woman was Madre Zuana, the founder of a hospital for the poor and sick. In addition, Kuntz has identified some of Postel's hitherto unknown works by showing that Jehan Boulaese, previously considered a disciple, is actually a pseudonym for Postel himself. Attempts to illuminate Postel's early career are less successful. The lack of solid and direct evidence makes the assertions about his professional status in 1530, the popularity of his teaching, or his early relations with the Jesuits, less convincing.

Since the monograph is overwhelmingly concerned with detailing the facts of Postel's life, there is little attempt to look at the significance of his thought. The author dismisses the task of identifying the intellectual influences on Postel by claiming that such an endeavor would produce "no clear picture of Postel but only a compartmentalization of influence" (p. xii). The book concludes with the observation that Postel was "a man, in many respects, an enigma in his own century and also in ours" (p. 177). A second book is promised that will treat in detail his philosophy and its application to his other views. Let us hope that a clearer picture of Postel may emerge from that work to resolve part of the enigma.

DONALD J. WILCOX
University of New Hampshire

FRANK A. KAFKER, editor. *Notable Encyclopedias of the Seventeenth and Eighteenth Centuries: Nine Predecessors of the Encyclopédie*. (Studies on Voltaire and the Eighteenth Century, number 194.) Banbury, Eng-

land: Cheney and Sons, for the Voltaire Foundation. 1981. Pp. 252.

In his preface, Frank A. Kafker says that because he lacked the time and space to touch on the more than thirty encyclopedias that appeared in Europe in the seventy-five years before the *Encyclopédie*, he chose to have nine of them examined by specialists in French, German, and Italian literatures; history; and the history of science. Of these nine, seven "served as models or sources for the *Encyclopédie*" (p. 9).

The forty pages of Arnold Miller's "Louis Moreri's *Grand dictionnaire historique*" and the thirty-two pages of "Johann Heinrich Zedler's *Universal Lexicon*" by Peter E. Carels and Dan Flory provide the most thorough account of the predecessor encyclopedias treated here. After sketching Moreri's brief life, Miller presents salient facts of the *Grand dictionnaire historique*'s twenty-four editions published from 1674 to 1759, and summarizes two criticisms made of it by the Protestant writers Jean Le Clerc and Pierre Bayle. Moreri's twenty-second edition (1740) was directed by a certain Platel. The bulk of Miller's essay is an absorbing discussion of representative articles from this edition on history, geography, science, literature, philosophy, religion, and government. It is to be hoped that Miller will soon find occasion to offer a more complete study of Moreri's encyclopedia. Of Moreri's editors, it can be said that "the long life of the work testifies sufficiently to their success" (p. 47).

The two authors of the article on Zedler's *Universal Lexicon*, which comprises sixty-eight volumes including the *Supplement* (1732-54), devote about one-fourth of their essay to Zedler's arduous struggles, eventually successful, to obtain publishing privileges in various German states, raise money for publication, find a reliable publisher and a competent editor, and locate knowledgeable contributors. Except for two pages on format and style, the authors' last twenty-two pages describe typical *Lexicon* articles in the areas of arts and letters, science, technology, medicine, geography, history, government, genealogy, religion, and philosophy. It is claimed that "with all its faults, the *Universal Lexicon* is a valuable reference work even today" (p. 195).

Among the other pieces in Kafker's modest collection, Lael Ely Bradshaw's "Ephraim Chambers' *Cyclopaedia*" and Silvano Garofalo's "Gianfrancisco Pivati's *Nuovo dizionario*" stand out as most informative. From the first edition (1721) to the tenth a quarter-century later the *Cyclopaedia* was a resounding success. Carefully edited, its two volumes contain countless cross-references, most of them multiple references; a genealogical tree in the preface and an index at the back are also included, as are many bibliographical sources and numerous plates and fold-out tables within the text. Chambers high-

lights articles on philosophy, science, technology, religion, and politics, at the expense of economics, history, geography, arts, and literature, only briefly noted.

The ten-volume *Nuovo dizionario*, published in Venice (1746–51), stresses physical sciences, natural history, anthropology, and only touches lightly on literature, music, and the other arts. Each volume has at the back a section of plates, most of these relative to anthropology, with a number of them illustrating flora, human and animal anatomy, architecture, and technology. One topic discussed at length by Pivati is commerce, another is magic and witchcraft, still others are on the history and status of Jews, the Inquisition, and the Church's position vis-à-vis topics made controversial by the appeal to science and reason as superior to tradition and authority. Given the repressive clergy and government of eighteenth-century Venice, Pivati's moderate and unbiased viewpoint is as remarkable as his encyclopedic achievement carried on almost single-handedly.

This study's chief value, we would think, could lie in encouraging researchers to undertake further and more detailed examination of the *Encyclopédie's* most significant European predecessors. The importance of such undertakings in turn would be measured in terms of the degree of the Enlightenment's penetration in the states and countries where these encyclopedias were published.

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HORST DIPPEL. *Individuum und Gesellschaft: Soziales Denken zwischen Tradition und Revolution: Smith—Condorcet—Franklin*. (Veröffentlichungen des Max-Planck-Instituts für Geschichte, number 70.) Göttingen: Vandenhoeck und Ruprecht. 1981. Pp. 269. DM 59.

With a bibliography of almost six hundred titles, two-thirds of them in English, this *Habilitationsschrift* is a well-argued contribution to both comparative history and the history of political thought. Horst Dippel reconstructs the process whereby the principles of the French and English Enlightenment were whittled into a consensus about social order, a consensus that sustained liberals throughout most of the nineteenth century. Liberals as varied as François Guizot, Friedrich List, and William Gladstone took for granted the categories whose origin Dippel traces in rich detail. This book is valuable above all as an account of the streamlining of Enlightenment ideas into the consensus that became nineteenth-century liberalism.

Dippel expounds Adam Smith, the Marquis de Condorcet, and Benjamin Franklin as repre-

sentative of a bourgeois world-view that coalesced in the second half of the eighteenth century. Eighty pages on the tradition of merchant moral philosophy in England precede one hundred pages on French variations on English themes and forty pages on North American implementation of the same categories. Terms like freedom, individualism, interest, equality of opportunity, property, work, virtue, and education were first emphasized by English merchants. Dippel interprets the moral philosophy of Adam Smith as the culmination of ideas that English merchants and philosophers had elaborated for several generations. Smith elevated these concepts to the status of common sense.

The same or similar categories met a very different fate in France. After being imported from England by the circle around Vincent de Gournay (who introduced the notion of *laissez faire*), these ideas circulated in Paris salons, where they inspired debates utterly divorced from economic reality. French controversies may be more stimulating because of their claim to universality, but they deprived the notions of "work," "virtue," and "interest" of their roots in the merchant class. In France the slogans of individualism emerged to protest an irresistible drive toward centralization (*étatisme*), while in England they served to defend a way of life already in place.

Dippel views North America as utterly unlike France in its manner of receiving English mercantile categories. Whereas France lacked an institutional basis for a social order based on individualism, North America lacked any interest in theorizing. North Americans embodied what Smith and his forbears had preached; the French merely disputed about such matters. Although Dippel devotes more space to French subtleties than to American practicalities, he seems to prefer the latter. It is rare to find any historian of ideas praising a preference for practice over precept, and when that historian is as well read (in English, French, and German) as Horst Dippel, the result is well worth pondering. Adepts of J. G. A. Pocock will find Dippel's application of his ideas especially provocative.

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ARTHUR L. GREIL. *Georges Sorel and the Sociology of Virtue*. Washington: University Press of America. 1981. Pp. xi, 249. Cloth \$20.75, paper \$10.50.

JOHN STANLEY. *The Sociology of Virtue: The Political and Social Theories of Georges Sorel*. Berkeley and Los Angeles: University of California Press. 1981. Pp. 387. \$30.00.

Georges Sorel (1847–1922) was the analogue in philosophy of the painter Henri Rousseau (1844–

1910). Both were retired civil servants whose highly eccentric, brilliant creations astonished and excited small circles of younger contemporaries—as well as surprisingly many who came later. In addition to the two volumes reviewed here, there has been a recent outpouring of academic works on Sorel and the circles he excited; most notable is Jack Roth, *The Cult of Violence: Sorel and the Sorelians*, published in 1980 by the same press as John Stanley's book.

Although Roth gives a very detailed, learned examination of the historical context of Sorelian thought, John Stanley and Arthur L. Greil employ essentially ahistorical approaches. Both sketch the historical context briefly, but they know relatively little about it and evidently do not think additional knowledge would be helpful. With good reason each of them believes Sorel's theory to have been grossly misunderstood and undervalued, and each in his own way wants to enrich social and moral theory by giving Sorel his due. In his own way each has achieved substantial success within self-imposed limits. Yet these limits keep either book from being what it could have been.

Sorel's thought was always conceived in terms of what was happening around him. He created new ideas through radical criticism of contemporary culture, society, and politics, and he usually expressed his thought in response to what someone else had said. Sorel was so thoroughly embedded in "historical context" that wrenching his ideas from that context risks twisting them into drastically different shapes.

Moreover, he cared little for being systematic or even consistent. Anyone interpreting and explaining these ideas is bound to lend them a kind of order beyond that made explicit by Sorel himself. This order can be derived from recognition of the historical and personal conditions that shaped his thought, and through adaptation of an analytic framework important in the interpreter's own intellectual discipline. Primary reliance on the latter course, however, also is likely to leave interpretation far from what Sorel intended.

Stanley adapts his analytic framework from political science and Greil takes his from sociology. Others have employed these disciplines to explore theory while developing sound appreciations of historical context, and theory is enriched thereby. Lacking such appreciation, each of the books reviewed here perceives from too great a distance and distorts by taking things out of context. Although not faithful to the original, the authors can still enrich theory through independent critical evaluation of what they see in Sorel's ideas, especially if they are able to develop theory beyond such criticism. To some extent they do this, but academic convention seems to bind them too closely to the texts for the degree of independence desirable here.

Of the two books, Stanley's is far more ambitious, refined, and long. Yet Greil does more to develop theory from Sorel; for the most part Stanley confines himself to explication of texts. Greil is straightforward and often persuasive. Stanley is more subtle and elegant, but he neither captures the spirit and fire that animated Sorel nor provides a substitute from his own concerns. Without either, his book seems too much like the complex of lifeless abstractions that academic political philosophy is often accused of being.

Yet Stanley's study of Sorel's theory is probably wider in scope than any other yet published. He illuminates important aspects of this theory that others have generally ignored or underestimated. With both these aspects and more widely familiar material, Stanley stimulates us to look at the subject afresh, even though we suppose him not to be faithful to the original.

The entire corpus of Sorel's work is very large and rather chaotic. Stanley takes nearly all, drops what he cannot abstract into a coherent whole, and presents the resulting theory as a grand, developing, original, and quite sophisticated vision of morality and vitality achieved through dynamic social interaction. Greil similarly interprets Sorel's conception of the intertwining of morality with social structure and dynamics, but his scrutiny is more narrowly focused, omitting or not much emphasizing matters that Stanley looks at closely, such as Sorel's epistemology. Although Greil lacks the other's scope, he more clearly and compellingly shows why Sorel is worth anyone's attention and respect.

Scholars advanced in the study of Sorel may find Stanley's monograph the more helpful, despite its limitations. Others—whatever their disciplines—can better turn first to Greil or to other authors. Sorel himself despised the abstracting intelligence of social science: he would have been appalled by both Stanley's book and Greil's.

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ELLIS SANDOZ. *The Voegelinian Revolution: A Biographical Introduction*. Baton Rouge: Louisiana State University Press. 1981. Pp. xiv, 271.

Eric Voegelin's contribution to modern political theory and philosophy of history is becoming increasingly recognized both for its magnitude and for its unique perspective, in which reality is conceived not as a set of entities but as a pluralistic experiential field characterized by a ubiquitous bipolar tension. The caricature of Voegelin as a "conservative" thinker remains possible only when one fails to notice the radical newness of this frame-

work of thought, which Ellis Sandoz describes as constituting "a revolution in the science of man comparable in magnitude (if not in style) to the revolutions of Copernicus and Newton" (p. 188). Sandoz offers a biographical approach to Voegelin's works, tracing the unfolding of his special vision of man and history as it emerged over several decades and gradually took on its currently evolving form in Voegelin's masterwork, *Order and History* (1956–). During this period Voegelin became acquainted with both European thinkers (such as Max Weber) and American ones (Sandoz offers especially valuable comments on the affinities of Voegelin's thought to that of William James). Within this biographical framework Sandoz gives good summaries of all of Voegelin's principal writings.

Sandoz's focus on the biography is, however, more than a way of organizing this body of material; it is fundamental to his own perspective on Voegelin, whom he considers as important as a model philosopher as he is as a theorist and historian of thought. Just as Voegelin's thought constitutes a fundamental shift in the paradigms of science, so Voegelin is himself, Sandoz believes, paradigmatic as an "exemplification of the contemplative life" and the "openness to reality" upon which his philosophy centers (pp. 200–01).

In order to document this biographical approach, Sandoz collected twenty-seven hours of taped interviews that were subsequently transcribed and edited into an "Autobiographical Memoir" of over one hundred twenty-five manuscript pages. Excerpts from this document make up perhaps one-fifth of this book and offer immensely valuable comments by Voegelin himself on his life and thought. Especially interesting are his observations on stratification in English usage (p. 76); on the theoretical limitations of his own use of "gnosticism" as an analytic concept and the need for a more differentiated terminology (pp. 78–79); on his shift, between volumes 3 and 4 of *Order and History*, from a unilinear to a pluralistic conception of history; and on his philosophy of consciousness and his theory of symbolism (pp. 170–01). This memoir alone would be sufficient reason why no one interested in Voegelin would wish to be without Sandoz's book.

EUGENE WEBB

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G. V. SCAMMELL. *The World Encompassed: The First European Maritime Empires, c. 800–1650*. Berkeley and Los Angeles: University of California Press. 1981. Pp. xiv, 538. \$35.00.

This handsome volume comprises nine separate essays dealing with the Norse, Hanseatic, Venetian, Genoese, Portuguese, Spanish, Dutch, French, and

English maritime empires up to 1650. Brief comparisons and contrasts do something to connect the essays with each other, but only slenderly. The brief introduction and conclusion merely define the book's subject matter, without attempting any analysis of European expansion as a whole.

G. V. Scammell has read widely and summarizes what is known about each of the empires he discusses with judicious detachment. He is sensitive to the technology of sailing ships and has instructive things to say about improvements in ship building and navigation as they occurred. On the other hand, Scammell's economics are descriptive rather than analytical and leave out most of the interesting questions about when and why each empire developed.

Overall, I found the volume oddly disappointing, despite the virtues of each essay taken separately. The format the author adopted involved considerable duplication. The Venetian and Genoese as well as the Dutch, French, and English overseas empires arose simultaneously after all. Treating each by itself therefore required Scammell to go over the same time span twice in the medieval centuries and three times in the early modern era. Treating the Hanse apart from the Mediterranean-based Venetian and Genoese empires is more successful, inasmuch as the geographical separation of the respective enterprises was real enough to make them effectively independent. The Iberian sea empires were intermediate in this respect, for they changed the economy of Europe jointly while acting separately (for the most part) overseas. By far the most successful part of the book is the first chapter, dealing with the Norse, who stand alone chronologically. Consequently, this chapter escapes the partiality of outlook implicit in writing about a single maritime empire in an age when several such organizations were simultaneously active.

A second and closely related defect of the book is the absence of any serious discussion of how and why one empire gave way to its successors. To treat this would require a larger canvas: one in which Europe as a whole would be background and matrix, and in which the competition and complementarity among the empires under discussion would constitute the focus of investigation. I conclude that the history of European expansion overseas is more—a great deal more—than the history of a series of separate sea empires. Scammell does not treat this larger whole, thereby generating the sense of disappointment I had in reading his work through.

This seems all the more regrettable inasmuch as his book, by spanning the conventional chronological break point at 1500, implies that Europe's medieval and early modern sea ventures were indeed part of a single process. Certainly the conventional

view that capitalism began in the sixteenth century deserves correction in the light of Italian and German commercial practices from the twelfth through the fifteenth centuries, and a book like Scammell's makes this unmistakably obvious.

Within the author's chosen compass, however, each essay constitutes an admirable summary of current scholarship. There is little in the way of original emphasis so far as I can tell, though once in a while Scammell lapses into a more personal voice. He refers, for example, to an "inescapable cycle of empire in which the defence of what has been cheaply and relatively easily acquired becomes an exorbitantly expensive duty" (p. 364). Inescapable or not, such a remark from a Cambridge don aptly describes Great Britain's current involvement with the Falkland Islands.

On a few matters, Scammell's views are at odds with my own. He attributes Spanish success in the Americas to cavalry, for example, not to disease. But most of what he has to say is unexceptionable—no small feat for a work covering as much ground as this one does. *The World Encompassed* is therefore a good place for anyone to begin a study of European overseas expansion. The book's very limitations are likely to provoke a reader to wonder what larger structures and processes were at work: and in such wondering lies the best of all possible stimuli to students of history.

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SIDNEY POLLARD. *Peaceful Conquest: The Industrialization of Europe, 1760–1970*. New York: Oxford University Press. 1981. Pp. xii, 451. Cloth \$49.95, paper \$19.95.

Sidney Pollard divides the industrial conquest of Europe into three stages. The first, to 1880, took place on the British model, and the essential element was technological. Location, resources, production methods, trade policies, and other "nourishing factors" determined the ways in which different regions undertook industrialization. Pollard's regional analysis helps explain why even in some highly industrialized nations certain areas remained "backward" and why there could be loci of industrial development in otherwise nonindustrialized countries. In this first stage the role of the government was largely irrelevant, and the new technology was transferred whole from Britain. This imitative transfer was possible because several regions in Europe possessed similar factor endowments and equaled in economic and technical preparedness those British areas that were first industrialized. By the end of this first stage, Britain had equipped its rivals with industrial machinery to the extent that

they were better able, because of lower wages, to make use of the new technology. Britain was driven out of European markets toward colonial trade while imports from the newly industrializing countries were the most rapidly growing item in Britain's own trade balance.

During the second stage of European industrialization, from 1870 to 1945, the "periphery," that is, the previously nonindustrialized sections, began to industrialize. In this new phase the nation state played a leading role. Between the 1870s and 1914 the rise of nationalism meant that national frontiers became economic barriers. Neomercantilist protectionism rapidly spread after the 1870s, and when political factors dominated over economic rationality it led to wider diplomatic conflicts. True, the logic of the industrial process forced some interdependence, such as international postal, telegraphic, and copyright agreements, but nationalistic interests gradually took precedence and helped bring about World War I.

That war led to even greater control over economic affairs by governments. The peripheral countries sought to create an industrial base as part of their quest for national independence: this was economically unrealistic, resulting in high-cost, inefficient, technologically backward industries that survived only by protection and official support. With the Great Depression the European economy disintegrated under the impact of tariffs, quotas, and outright prohibitions of trade. Pollard claims that Soviet Russia was the only country to industrialize successfully in this period, but makes no mention of Russia's reliance on foreign technology for its "success."

In the last portion of his book Pollard traces the third stage, the post-World War II phase, of Europe's industrialization, when a new conflict developed as the Marxist-communist world challenged Western hegemony and as Europe split into two parts, with economic integration existing within each of the separate parts.

The Eastern European countries attempted to be small-scale replicas of the Soviet model rather than interdependent and complementary economies, and Pollard concludes that their industrialization was spectacularly successful. As a result, the industrialization process, which began two hundred years ago in a few remote counties of Britain, has now covered the complete European continent, although Pollard admits that the price paid is that there are now two Europes, not one.

If Pollard had continued his historical inquiry beyond 1970, one might wonder if Eastern Europe's industrialization would be regarded as such a resounding success. Instead, the failure of most of these countries to provide their citizens with adequate food, clothing, and shelter might lead one to

suspect otherwise. Indeed, one might be tempted to say that the industrialization of Europe was not quite the "peaceful conquest" of Pollard's title. Or perhaps the "conquest" is at best a Pyrrhic victory.

Nevertheless, Pollard draws from his study of European industrialization some useful lessons, which might be helpful to Third World countries today. He shows that although the technical means can be copied with only minor modifications, the industrialization process is never repeated in identical fashion. The timing is all-important, as is the role of governments, and the lessons of the past must be "applied with genuine historical understanding." His book contributes to that historical understanding.

MELVIN KRANZBERG

Georgia Institute of Technology

JOHN R. HANSON II. *Trade in Transition: Exports from the Third World, 1840–1900*. (Studies in Social Discontinuity.) New York: Academic Press. 1980. Pp. xii, 197. \$18.00.

This is an empirical study of exports from the less developed countries (LDCs) during the nineteenth century. It challenges current theories about the importance of foreign demand for the economic development of Third World countries. The study is based on official English and American trade data for the period 1840–1900 but ignores statistical sources published in West European and Third World countries. It has chapters on general trends in nineteenth-century trade; on the diversification, concentration, and geographic distribution of exports from Asia, Africa, and South America; on determinants of LDC exports between 1820 and 1900; and commodity histories for cotton, silk, sugar, tea, copper, rubber, and so on. A brief final section covers tariffs, exchange rates, and terms of trade. John R. Hanson II finds that the growth of the world economy in the nineteenth century was not rapid enough to bring about improvement in the economic prospects of the LDCs. Furthermore, he says, the rate of growth of LDC exports declined after 1860 and there was near stagnation in the last quarter of the nineteenth century as the underdeveloped countries faced increasing competition from the specialty exports of the industrial countries. "While non-British demand for LDC goods was rising faster than British demand, it was not rising fast enough to sustain the aggregate LDC exports achieved during the first half of the century." Hanson questions then both "export-led" theories of growth that stress the importance of world demand for the development of the LDCs and current neo-Marxist theories (Prebisch, Amin, Emmanuel, and others) that attribute the backwardness

of the LDCs to exploitation by the (now) developed countries.

The author has done an impressive quantity of work and has made a serious and informed contribution to the literature on trade and development. The study, however, raises a number of questions on technical grounds. Hanson studies exports for only bench-mark years (1840, 1860, 1890, 1900) but excludes imports and thereby the possibility to study the international balances that were the true measure of the relative importance of the LDCs in the international economy; price indexes for LDC exports are based on the Schlote-Imlah import price series for Great Britain, not on current export prices. The Schlote series, as is well known, has limited validity for the first half of the nineteenth century because of its restricted coverage. Moreover, his conclusions concerning the deceleration of growth rates of LDC exports depend actually on base-year weights—had these been shifted forward to 1913, the results might have been drastically different; also export values are expressed in current dollars, which are somewhat misleading indicators of real values given falling exchange rates and devaluation of silver during this period. Finally, too little attention is paid to business cycles, which had a profound effect on secular trends in international trade in the nineteenth century.

If trade was truly the engine of growth for the LDCs, its effect would depend on its size and its multiplier. Hanson argues that since the export sector was small and the rate of growth of per capita exports low, there was little export diversification and no internal structural change in the LDCs; thus export-led growth could not have occurred. It is nevertheless important to point out that the size of the export sector tells us little about its effect on the overall growth of the economy. Hanson also concludes that the industrial countries did not expropriate LDC products or raw materials because the growth of LDC exports was decelerating—but this involves a form of circular reasoning.

There is another dimension, however, to the problem. Arthur Lewis has recently analyzed the failure of growth in Third World countries in the nineteenth century. Development, he has pointed out, depended on two vast streams of international migration—fifty million emigrants left Europe for temperate settlements in the nineteenth century, while an equal number left India and China to work on tropical plantations and mines. The availability of these two streams of labor set the terms of trade for tropical and temperate agricultural commodities. In the temperate zone high prices attracted European migrants. But in the overpopulated tropics, wages had only to sustain indentured Indians. These wage differentials were rooted in differences in agricultural productivity. Thus where trade of-

ferred high per capita incomes and stimulated demand for manufactured products and urbanization in the temperate settlements, it offered the tropics only the opportunity to stay poor because of the vast labor reserves of India and China. Industrialization at the center offered very little to the periphery.

One wishes that Hanson had considered some of these factors in this ambitious study of LDC exports.

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JAMES BENNETT. *Oral History and Delinquency: The Rhetoric of Criminology*. Chicago: University of Chicago Press. 1981. Pp. xv, 363. \$28.50.

This interesting book analyzes the ways in which oral history has been applied to the study of delinquency in the Anglo-American world since 1800. James Bennett traces the tradition from Henry Mayhew, whose *London Labour and the London Poor* (1851–52) introduced the “own story” approach, to the Chicago sociologists who hoped that elicited autobiographies, such as Clifford Shaw’s *The Jack-Roller* (1930), would supplement and popularize their ecological studies. Bennett emphasizes the generally conservative character of the genre despite the “marginal” status of both the transcribers and their subjects. In the interest of reducing class tensions, Mayhew and his successors used their observations of slum life to acquaint affluent audiences with the human qualities of “the dangerous classes.” But familiarity only bred complacency or even resentment, since some accounts romanticized lives of deprivation and crime. A dissenting tradition, epitomized by Judge Ben Lindsey’s “snitching bees” and the antipoverty programs of Saul Alinsky, sought with little success to publicize the political forces that created and maintained inequitable socioeconomic conditions.

There is much to consider here. Supplementing the works of Burton Bledstein and Thomas Haskell, Bennett perceptively delineates the conservatism of the professionalization process as reflected in changing styles of life history. Thus William Healy, founder of the first juvenile psychiatric clinic, turned stories into diagnostic case studies, a trend Bennett links to a similar development in law schools. Healy used his new role as an “objective” scientist to minimize political factors. Relatedly, Bennett concludes a stimulating analysis of William I. Thomas’s *Polish Peasant in Europe and America*, by noting that contemporary sociologists have repudiated “continuity of process as a virtue of life histories” in their “onrush to split reality into pieces more easily handled in research projects” (p. 137).

Since his subject is participant-observer relation-

ships, Bennett is particularly obligated to explicate his own point of view and to explore all of the dimensions of his various topics. He does not always succeed. For example, his criticism of Mayhew and his admiration of Alinsky is less than persuasive. He tiresomely berates Mayhew for lacking Marx’s power as a social analyst while he fails to pursue the linkage between Alinsky’s ideas and some of the programmatic shortcomings of the War on Poverty. Perhaps there is a connection between this querulous tone and Bennett’s reticence about his own position as program coordinator for the social sciences at the Office of Sponsored Research, University of Illinois at Chicago Circle. These are hard times for social-policy analysts who are sympathetic to poor people. Contemporary political pap, which utilizes isolated tales of welfare fraud to obscure massive transfers of wealth to the already wealthy, must be a special irritant. This makes all the more puzzling Bennett’s failure to examine radical modern testimonies such as John Osborne’s *Loneliness of the Long Distance Runner*, Brendan Behan’s *Borstal Boy* or even Jean Genêt’s *Miracle of the Rose*.

It may be true, as Bennett claims, that human documents cannot “by themselves validate a proposition” (p. 143). But does this necessarily lead to pessimistic conclusions? Commonwealth ideals may yet produce another leader like Franklin Roosevelt. And there was nothing marginal about his use of human-interest stories to rally support for democratic social programs.

Perhaps Bennett agrees, since he concludes on a hopeful note. Contemporary criminological study, he notes, is preoccupied with “systems of power and the institution of anti-rehabilitative measures of punishment” (p. 264). These approaches, however, obscure the “simple truths” of oral histories and hence create the audience for whom the individual account is “again new.”

This book deserves to be widely read and discussed.

ROBERT M. MENNEL
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CHARLES PRESS. *The Political Cartoon*. Rutherford, N.J.: Fairleigh Dickinson University Press or Associated University Presses, Toronto. 1981. Pp. 395. \$37.50.

The title, chapter headings, and introduction of this work promise a full-scale history of political cartoons, but the text fails to deliver. After a 22-page effort to define what a good cartoon is, a definition unfortunately built around what it is *not*, Charles Press suggests that the reader may “wish to close the book up here and go study Egyptian belly-dancers”

(p. 32). Would that a reviewer could in good conscience accept this advice! Subsequent chapters also promise pure gold while delivering dross. For example, "Birth and Development of Cartoon Symbols" is really only about Uncle Sam, with a side-glance at John Bull; "Early Prints and Magazine Cartoons" deals only with the United States and concentrates on Thomas Nast; "Other Democracies and Their Cartoons" covers mostly England, with a cursory glimpse at Australia and that paragon of democracy, South Africa. The strong French tradition of cartooning gets only a few vague paragraphs here while Germany, Italy, Israel, and Latin America—all lively cartoon arenas—are ignored.

The book's chief problem is that it purports to cover the entire history of political cartoons, although the author apparently has consulted no sources outside the English language. Nor has Press bothered to look at some key sources in English; most glaringly absent from the bibliography are the works of Mary Dorothy George, notably *Hogarth to Cruikshank, English Political Caricature* and the *British Museum Catalogue of Political and Personal Satires*.

Even within the narrow area that the book tries to cover there appears to be no underlying organization. The text and illustrations dance from subject to subject, from period to period, and from country to country with little point or logic (except to remind us that Russian cartoonists labor under "totalitarianism," while American cartoonists bask in "democracy"). Nowhere in this nervous buzzing does the author alight long enough to illuminate a style, a development, a point of view, or an individual artist. Rather, the illustrations mirror and supplement the mishmash in the text. Their least failing is the ugly gray background with which they are reproduced; this destroys the crisp contrast of the originals. Worse is the fact that many cartoons described in detail in the text are not reproduced at all. When they do appear, the illustrations are often many pages away from the pertinent text, with no references by number or page. Finally, the book lacks even a list of illustrations or a boldface guide in the index to indicate them.

The writing is equally slapdash. The author abhors "blah messages" or pictures that "ring phony" (p. 19) and confesses that he judges "arty art" more harshly than garden-variety cartoons (p. 17). Although he decries triteness in cartoons, he cheerfully unloads cliché-ridden prose: "Art that called a spade a spade and drew the enemy as a tool of Satan seemed to hit the nail exactly on the head" (p. 39). And though he urges clarity and originality upon cartoonists, he brashly spatters the reader with such sentences as "From this point on, it was, as it has been expressed by a now almost-forgotten sports-oriented president, a different ballgame" (p. 34).

On the positive side, there are probably some interesting or even original ideas about political cartoons hidden in this scattergun book, but they are artfully drowned in the "sea of molasses" in which the author confesses he is "swimming" (p. 368).

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DAVID KUNZLE. *Fashion and Fetishism: A Social History of the Corset, Tight-Lacing, and Other Forms of Body-Sculpture in the West*. Totowa, N.J.: Rowman and Littlefield. 1982. Pp. xxiii, 359. \$27.50.

The major thesis of David Kunzle's volume is interesting and well argued. In contrast to what has become conventional corset wisdom, Kunzle sees tight-lacing as a defiant attempt by women to challenge their appointed role of parenthood and family care. In so doing he challenges the view that corsets, like the Chinese practice of foot-binding, were a male plot designed to keep women in perpetual bondage by restricting their movement and, by analogy, their freedom. The corset was rather a symbol of liberation pointing to both sexual and companionate relations with men. Those who opposed corset culture tended to be very conservative males, who objected to the erotic nature of body sculpture even when they couched their antilacing arguments in terms of damage to health.

Although the argument is provocative, the book suffers from several weaknesses. The absence of material on tight-lacing in many centuries means that any general work will be uneven and disjointed. Kunzle is at his best in the nineteenth century, where the data are richest. Before that time the book has an unpleasant hunting-for-references quality that leaves the reader unsure of the importance of the lacing phenomenon in the past. Lack of evidence also leads Kunzle to give undue weight to magazine articles and medical reports. We are never quite sure of whether this literature signifies major lacing battles or minor skirmishes of little interest to nineteenth-century females.

Finally, Kunzle's book is not well organized. In the end the author's note cards seemed to have dictated the length and organization of the volume. The reader is left wandering without much rhyme or reason from *The Englishwoman's Domestic Magazine* to *La Vie Parisienne*, from analogies to masturbation and footwear to various equine devices. Perhaps this is inevitable when scholars write general books before a sufficient number of articles and monographs have prepared the way. Nonetheless, Kunzle has raised in a provocative manner the significance of female (and male) fashion and its

symbolic role in society. It is a topic that hopefully will interest other scholars who might want to explore smaller segments of this phenomenon in greater depth.

ARTHUR N. GILBERT
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JAMES TURNER JOHNSON. *Just War Tradition and the Restraint of War: A Moral and Historical Inquiry*. Princeton, N.J.: Princeton University Press. 1981. Pp. xxxv, 380. \$30.00.

This study continues James Turner Johnson's exploration of the just war, begun in his *Ideology, Reason, and the Limitation of War* (1975), reviewed in the *AHR* in February 1977. The new volume overlaps somewhat with the earlier, returning to the origins of Christian just-war doctrine in the writings of St. Augustine and Gratian, but carries the analysis forward to our own time. The first of the book's three parts outlines the theoretical and methodological context. Although occasionally difficult to follow, the discussion contains much of interest, for instance an analysis of sixteenth-century efforts to expand the just-war tradition of the West into a universal doctrine. The concept of natural law is employed to bridge ethnic and cultural differences between antagonists: it alone "fully takes account of cultural plurality," so long as it is "less a normative than a descriptive term for the way people in general [are] observed to act" (pp. 114–15).

The second and third parts of the book explore the relevance of the history of war and of attempts to restrain war for contemporary moral analysis. Regrettably the historical discussion does not support the analytic burden placed on it. Much of it is marked by three recurring weaknesses: (1) The changing reality of war is treated somewhat schematically. For example, while recognizing that medieval wars did not follow a single pattern, Johnson nevertheless emphasizes "social restrictions that . . . made warfare a class activity" (p. 188). But medieval wars were not solely or even primarily fought by knights—themselves a diverse, constantly changing social group—and "knightly concepts" were not as dominant as he appears to think. What he regards as consensus on proper behavior in war might be more accurately termed the ideals of an elite, reflected in myths designed to strengthen its self-image, which only imperfectly affected reality. (2) The distinction between theorists, soldiers, and political leaders is not drawn sharply enough: "Frederick [the Great] and his contemporaries simply took for granted their right to wage war, and this shows the real difference between his thinking and [Franciscus de] Victoria's" (p. 209). If the intention is to contrast sixteenth-century and eighteenth-

century attitudes to war, Frederick ought to be compared not with a theologian, but with another monarch—for instance with Victoria's contemporary, Philip II. (3) There is also reliance on inadequate sources: the confusing, largely erroneous treatment of Clausewitz's concept of "national war"—not a term Clausewitz employed—and its equation with absolute war derives perhaps from an edition of poorly translated selections of *On War*. The relevance to the author's arguments of this aspect of Clausewitz's theories is, in any case, doubtful. More to the point would have been to investigate the statement in the opening chapter of *On War*: "Attached to force are certain self-imposed, imperceptible limitations hardly worth mentioning, known as international law and custom."

The book regains conviction when it reaches the American experience. Johnson's discussion of regulations on the treatment of noncombatants during the American Civil War is original and enlightening, and his concluding chapter, "Just War and Contemporary War," with subtle analyses of Reinhold Niebuhr and Paul Ramsay, persuasively integrates ideas on war and pacifism with the political crises of the modern world that have given rise to them.

PETER PARET
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JOST DÜLFER. *Regeln gegen den Krieg? Die Haager Friedenskonferenzen von 1899 und 1907 in der internationalen Politik*. Frankfurt a./M.: Ullstein. 1978. Pp. 434.

The current nuclear freeze campaign underlines the urgency of analyzing earlier attempts to control arms races. In a judicious combination of diplomatic and peace history methods, Jost Dülfer investigates "the question of how peace was actually preserved in the period before World War One" (p. 9) and studies the alternative "models which were developed and discussed by the established elites in governments and especially in foreign ministries" (p. 10). Because they constitute the first modern attempt at disarmament (or rather arms limitation) "by slowing down or freezing armaments and renouncing certain weapon systems" or by "introducing peace-enhancing institutions such as arbitration courts" (p. 11), the Hague Peace Conferences of 1899 and 1907 provide an instructive case study of efforts to secure peace through a reform of the international system.

Unfortunately the experience of the Hague Conferences is discouraging. The wider aim of disarmament was excluded before the talks between the powers began, and even the limitation of armaments by defining weapons qualitatively, limiting

them quantitatively, or prohibiting new technology foundered on the rocks of verification, national sovereignty, and definitional precision. Only some minor weapons, such as dum-dum bullets, were proscribed. Despite the lack of enthusiasm of most diplomats for the Russian initiative, the public pressure of the international peace movement forced the conferences to achieve some concrete results concerning arbitration and mediation. These eventually led to the establishment of the International Court of Justice at The Hague and the conclusion of a number of bilateral arbitration treaties for matters not touching on honor or national interest. This "generally negative balance sheet" (p. 236) was partly the responsibility of German intransigence, which emphasized "the absolute sovereignty of the state" and claimed "the position of a true world power" anticipatorily (pp. 345–46). Although the Europe-dominated world system was complemented by the "full establishment of a separate American (sub-) system around the turn of the century" (p. 337), its self-regulating mechanisms began to break down with increasing polarization between the emerging power blocs. Not surprisingly, the most destabilizing country was Germany because it increasingly moved toward "the possibility of a 'revolutionary' break-up of the system out of weakness" (p. 341).

Exceptionally well written for a *Habilitationsschrift*, Dülffer's book largely realizes what this reviewer recently recommended, namely a critical synthesis of diplomatic and peace history approaches. The documentation is vast, including not only German, but also British, American, and other sources. Despite coming from a somewhat traditional academic environment (A. Hillgruber), the author is open to the critical impulses emanating from the Fischer school, H. U. Wehler, and others. His probing analysis of the political self-interest behind peace rhetoric not only debunks cynically, but also gives idealist motives their due. Some repetitiveness in the midsection of the book, some all-too-easy nominalist clichés (social imperialism used unreflectively), and some reluctance to discuss the second conference fully (where it transcends Dülffer's immediate purpose) are minor annoyances. In the interest of understanding the failure of realistic peace initiatives in the past one can only hope that other historians will follow this balanced and informative example.

KONRAD H. JARAUSCH
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J. L. TALMON. *The Myth of the Nation and the Vision of Revolution: The Origins of Ideological Polarisation in the Twentieth Century*. London: Secker and Warburg or University of California Press, Berkeley and Los Angeles. 1980. Pp. xviii, 632. \$35.00.

This volume completes J. L. Talmon's trilogy and life's work on "the messianic salvationist drive released by the French Revolution" (p. 552). It spans the first century of interaction between diverse nationalisms and Marxist internationalism. Its coverage, confined to Europe, is spotty even there: Marx and Engels on nationalism, Marxist revisionism in Germany ca. 1900, national frictions in Austria-Hungary's socialist ranks, Jewry and world revolution, Russian socialism before Stalin, Fascism, Nazism. But its brief "Conclusions" range far and wide.

Talmon's only tenable findings are unoriginal. His master discovery that Marxism and nationalism have tended to work at cross purposes is no news. Neither is his realization from an exhaustive reading of Marx and Engels on the nationality question that they "never in fact came to grips with" it (p. 64). Conversely, he overinterprets the turn-of-the-century conflict within the Second International over updating or even outdating Marx when he reduces it to a debate between Eduard Bernstein and Rosa Luxemburg for and against the national principle in history. But then he recurs to common knowledge to retell how ethnic conflicts among socialists in the Habsburg domains gave the lie to proletarian solidarity there. His excursus on the Jewish input into internationalist revolutionism concludes unsurprisingly (after a rundown of the troubled relations between Jew and gentile from the Covenant to the Holocaust) that the threat to Jews from the nationalisms of Europe helped incline them toward internationalism—or, to give this Talmon's gratuitous Freudian twist, that they fought their own discomfiting Jewishness through the fight against nationalism in general. Sketches of the revolutionary intellectuals Belinsky and Herzen, Chernyshevsky and Dobroliubov, Bakunin and Lavrov, Tkachev and Nechaev lead up to a portrait of Lenin as the treasonous heir to their patriotic defiance of tsarism, who thrust his backward country into the forefront of the universal self-destruction of capitalism only to split the Socialist International in the homespun expectation that foreign comrades were as alienated from their respective national regimes as any Bolshevik was from tsarism. Actually Lenin seized power from a republican government, not from the tsars, and he rejected the legacy of the Russian intelligentsia with cold contempt, whatever his fondness for Chernyshevsky's puerile *What is to be Done?* Talmon's Mussolini, for his part, is heir to Georges Sorel, whom the historic Mussolini most probably never read and most certainly never followed. Talmon's account of Hitlerism, with *Mein Kampf* as sole primary source, turns on the misconception that Hitler rose to power by beating the antisemitic drum that in fact he muffled for that very purpose. The long and short of

Talmon's "Conclusions" seems to be that power politics have denatured Marxism and nationalism of late without destroying either. But here as elsewhere there may well be more to Talmon's meaning than comes through his cluttered prose.

The index contains only proper names, with just page numbers even for Marx and Lenin. The notes spread needlessly over seventy pages and, in the absence of a bibliography, list obvious secondary works at intervals while passing over others equally obvious. They sustain the running effect of hand-me-down learning unequal to its vital subject: two political and emotional allegiances tearing our world apart.

RUDOLPH BINION
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ROBERT J. ALEXANDER. *The Right Opposition: The Lovestoneites and the International Communist Opposition of the 1930s*. (Contributions in Political Science, number 54.) Westport, Conn.: Greenwood Press. 1981. Pp. x, 342. \$32.50.

Except for this journal's handful of readers who are specialists in the intricacies of factional splits of the far left from the Bolshevik revolution until about 1940, the title of this volume is likely to be puzzling. This is not a work, however, to be dismissed as so narrow as to be useful only for sectarians. Historians of North America, Europe, Argentina, Mexico, and India whose studies relate to any of the many varieties of Marxist politics, including social democrats, will find clarifying information in this book.

One of the troublesome tasks facing the Comintern almost from its earliest days was to mediate the claims of competing groups within the communist movements of other countries to be the true Bolsheviks of their nations and to gain thereby official recognition, the "franchise," from the Soviet party. This problem existed even before Lenin's death in January 1924, but it became exacerbated and further confused by the complicated struggle for succession within the Soviet Union. By the late 1920s, after years of complex maneuvering and shifting alliances, Stalin emerged as the victor. The factions of the various communist parties beyond Russia that supported Stalin soon thereafter gained the official recognition they had long sought. Many of the losers in this long and tangled process, however, still believed themselves the true Bolsheviks. The Right Opposition—and to a lesser extent the Left Opposition, or Trotskyists—continued to regard themselves as an integral part of the international communist movement and did not withdraw from the Comintern until the late 1930s. Robert J. Alexander, a political scientist at Rutgers University who has written voluminously on Latin American mat-

ters, explains it succinctly: "Fundamental to the creed of the International Communist Opposition and its affiliates was the idea that they were 'Communist Oppositionists.' It was not a political group in competition with the Communist International and its national parties, but rather, was a 'faction' of the International Communist movement, outside of the Comintern for reasons beyond its control, and anxious to return . . . when and if the [Comintern's] policies were changed and [its members] could advocate freely the positions which they supported" (p. 9).

Nearly half this book is devoted to the American Right Opposition, led by Jay Lovestone and usually known as the Lovestoneites. The Lovestoneites were alarmed by Stalin's purges, especially Nikolai Bukharin's trial in 1938, but they did not completely repudiate the Comintern until after the Nazi-Soviet Pact. In December 1940 they announced their dissolution, a highly unusual action in left-wing politics. Good sources for the Lovestoneite movement are abundant, and Alexander has mined them well. Unfortunately, sources for the Lovestoneites' counterparts elsewhere in the world are scarce and scattered. The author was forced to use the frequent reports in Lovestoneite newspapers about developments in their colleagues' organizations quite heavily, and he supplemented these with documents of the International Communist Opposition (founded in 1930), interviews and correspondence with former Right Opposition leaders, and secondary works.

DAVID A. SHANNON
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LUCY S. DAWIDOWICZ. *The Holocaust and the Historians*. Cambridge: Harvard University Press. 1981. Pp. x, 187. \$15.00.

The Holocaust—the Nazi murder of the European Jews—has been the subject of increasing numbers of historical studies and is taking its place alongside the traditional fields of Nazi Germany and World War II. Therefore it deserves historiographical treatment, but Lucy S. Dawidowicz's slim volume does not provide an acceptable scholarly analysis, for it is, instead, a polemic that uses its chosen theme merely as a point of departure.

Dawidowicz has a simple thesis, namely that "the murder of the European Jews [has been inadequately] recorded in the history books" (p. 1). In her attempt to prove this thesis she simply disregards all contrary evidence. She ignores the three scholarly histories of the Holocaust: Leon Poliakov's *Bréviaire de la haine* (1951), Gerald Reitlinger's *The Final Solution* (1953), and Raul Hilberg's magisterial study of *The Destruction of the European Jews* (1961). She

also ignores such crucial recent monographs as Uwe Dietrich Adam's *Judenpolitik im Dritten Reich* (1972), H. G. Adler's *Der verwaltete Mensch* (1974), and Christopher R. Browning's *The Final Solution and the German Foreign Office* (1978). Dawidowicz further ignores the major histories about the fate of the Jews in occupied Europe, including H. G. Adler's monumental study of Theresienstadt or Randolph L. Braham's numerous works on the Hungarian Holocaust. She omits entire countries, disregarding the works published by the Centre de Documentation Juive Contemporaine, the Rijksinstituut voor Oorlogsdocumentatie, the Dokumentationsarchiv des österreichischen Widerstandes, and the Leo Baeck Institute. She also disregards all works on refugees, rescue, resistance, and the response of governments and institutions, including monographs by Reuben Ainsztein, Gilbert Badia, Henry L. Feingold, Saul Friedländer, Meir Michaelis, Bernard Wasserstein, David Wyman, and Leni Yahil. Finally, she leaves out all works on the SS, police, and camps, including the *Anatomy of the SS State* by Hans Buchheim and others (1968); the studies of the SS by Gerald Reitlinger and Heinz Höhne; the books on the concentration camps by Elie A. Cohen, Benedikt Kautsky, Eugen Kogon, Eberhard Kolb, Hermann Langbein, and Hans Marsalek; and Adalbert Rückerl's *NS-Vernichtungslager im Spiegel deutscher Strafprozesse* (1977).

Although she omits all works dealing with the Holocaust, Dawidowicz nevertheless denounces American historians for their supposed failure to treat adequately the Nazi murder of the Jews. Her strictures have some merit when applied to textbooks, but there she only repeats the conclusions of earlier investigations (p. 151, n. 2). Unable to distinguish between a textbook and a specialized monograph, she berates historians for the "tunnel vision" of "specialization" (p. 29); in three giant footnotes (notes 17, 20, 21 on pp. 152–55) she rates monographs on Nazi history on the basis of her definition of "sensitivity."

The explanations for the supposed omissions of American historians are equally strange. Dawidowicz argues that "it was to be expected that non-Jewish historians would not approach the history of the murder of the European Jews with the same empathy and moral concern that would normally apply when writing the history of their own people" (p. 2). This explanation is myopic, objectionable, and simply wrong. It overlooks the contributions of Christopher R. Browning, John S. Conway, Gavin I. Langmuir, Telford Taylor, and others. Her additional explanations are equally unconvincing. She asserts that students of German history "formed lasting attachments to the country and its people" or "were bedazzled by the illustrious names of Humboldt, Ranke, Sybel, Meinecke" (p.

26), but she produces no evidence to support this accusation. She also asserts that "academic historians, who have experienced only the happy provincialism of American politics" do not treat the Holocaust because they "are not accustomed to dealing with ideology and with its political impact" (pp. 29, 30). But this absurd notion is contradicted by many studies in Russian, German, and Austro-Hungarian history.

Dawidowicz thus pronounces most historical approaches inadequate for studying Nazi Germany and the Holocaust. She disqualifies political, diplomatic, and military, as well as social and economic history (p. 30), but she also rejects psychohistory (p. 41). She only approves of works that deal with ideology and its impact. But even this approval is very qualified. She gives only half a paragraph each to the works of Norman Cohn analyzing historical psychopathology (p. 41) and those by George Mosse probing mass culture (p. 31); she dismisses all works on fascism and totalitarianism, including those by Franz Neumann and Hannah Arendt, as "cosmic interpretations" (p. 58 and p. 160, n. 25); and she banishes all studies of antisemitism to one large footnote (p. 180, n. 35).

The treatment of British and German historians is equally misleading. She discusses the "shallow treatment" by three British historians of the Jews under Nazi rule: Alan Bullock is faulted for writing a "biography . . . of the old school" (p. 32); A. J. P. Taylor is criticized for "contempt for ideas as a motivating force" (p. 33); and Hugh Trevor-Roper is alleged to share the "disdain with which members of the English upper class usually regard those they consider their inferiors, the Jews being such a category" (p. 34). She likewise attacks three German historians, using well-known arguments to dismiss the early postwar works of Friedrich Meinecke and Gerhard Ritter as "apologia" (p. 57), but uses *ad hominem* arguments to discredit Hans Rothfels as a "converted Jew" (p. 61). She attacks Rothfels for his "preferred stance of noninvolvement" (p. 62) as editor of the *Vierteljahrshefte für Zeitgeschichte* but never mentions his publication of the Gerstein Report in the first volume of the journal. She acknowledges the contributions of the Institut für Zeitgeschichte, but argues that the "style of calculated objectivity" in its publications "suggest[s] an attitude of moral disengagement and a state of emotional anemia" (p. 63). Is emotionalism the hallmark of solid history?

In two chapters on Poland and Russia, the author treats serious scholars like Karol Marian Pospieszalski only in passing (p. 102) and deals mostly with antisemitic publicists like Tadeusz Bednarczyk (p. 116). The discussion is just a polemic on the well-known subject of postwar communist antisemitism and anti-Zionism.

Finally, Dawidowicz's analysis of "The Holocaust in Jewish History" (chap. 6) is disappointing. Omitting Israeli publications as well as all compilations of documents by historians like Josef Wulf or Helmut Eschwege, she restricts herself to a few historians of East European Jewish history, citing with approval no more than the works of Philip Friedman and Isaiah Trunk of the Yivo Institute. More important, she never applies the rigorous test imposed earlier on works in modern German history to those in modern Jewish history. Her weak explanation for the inadequacies of Jewish historians in dealing with the Holocaust is that the subject itself is "still too close to trauma" (p. 135).

It is unfortunate that a reputable press like Harvard issued this not very helpful volume, thus possibly foreclosing the publication of a serious historiography of the Holocaust.

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MARK R. ELLIOTT. *Pawns of Yalta: Soviet Refugees and America's Role in Their Repatriation*. Urbana: University of Illinois Press. 1982. Pp. xiii, 287. \$17.95.

Between 1945 and 1947 the Allies repatriated over two million displaced Soviet nationals. A significant number of these displaced persons were returned to Russia against their will. Mark R. Elliott considers the forced repatriation of Soviet nationals to be second only to genocide in the catalogue of horrors brought about by World War II.

In the first part of this unusually passionate and well-documented monograph, Elliott traces the evolution of American policies toward the repatriation of the Osttruppen (the one million Soviet soldiers who fought for the Germans), POWs, members of labor battalions, and other Russian nationals who were found behind Allied lines in late 1944 and 1945. His careful analysis of new archival materials reveals that while some American officials were uncomfortable with the agreement reached at Yalta to send all Russians home, they were more concerned about the implicit quid pro quo—the safe and speedy return of the twenty-four thousand American servicemen caught behind Russian lines. As early as December 1945 the United States began to retreat from its unconditional pledge to return all Soviet citizens. Elliott estimates that as many as five hundred thousand potential repatriates were "saved" by a combination of a change in the American approach, clever ruses employed by Russian citizens, and bureaucratic obfuscation. Playing no small role in the changes in the administration of the Yalta policies were the violent, often suicidal, acts of resistance offered by some of the Russians.

Elliott's subtitle is misleading. In this book, which grew out of his dissertation, he goes well beyond American policy to include chapters on the socioeconomic backgrounds of the five hundred thousand nonreturners, the adventures of American servicemen caught behind Soviet lines, the brutal treatment meted out to those who returned to Russia, and an especially interesting chapter on Soviet historiography on this issue. One unfortunate result of this wide-ranging, nonchronological approach is a good deal of repetition once the diplomatic aspects are discussed in the first part of the book.

Elliott, a fierce opponent of totalitarianism and the Soviet system, strongly condemns the United States for inept and inconsistent diplomacy. Contributing to the debate about the origins of the Cold War, he depicts Roosevelt and Truman as weak leaders who never realized that they could (and should) have taken a tougher line on the repatriation issue. Indeed, he finds the unassertive Americans almost as guilty as the Russians of violations of human rights.

To be sure, the return of civilians against their will was unconscionable. Elliott, however, is almost as concerned about the Osttruppen and their ilk as he is about civilians. He suggests that even General Andrei Vlasov might have been a candidate for political asylum since he was an antifascist who allied with the Nazis in order to liberate Russia from the evil Soviet system. Further, Elliott was evidently unaware of the hundreds of Russian-born Nazi collaborators spirited off to safety by American intelligence agents in an operation described on "60 Minutes" (May 16, 1982).

Pawns of Yalta is a provocative, well-researched study that raises a variety of important political and philosophical questions. Regardless of whether one accepts all of his arguments, the author has clearly demonstrated that the forced repatriation of Russian nationals was one of the most distasteful episodes in the history of the Second World War.

MELVIN SMALL
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KENNETH W. THOMPSON. *Cold War Theories*. Volume 1, *World Polarization, 1943–1953*. Baton Rouge: Louisiana State University Press. 1981. Pp. 216. \$18.95.

This is the first part of a two-volume study of the Cold War. It is stated in the blurb that Kenneth W. Thompson "evenhandedly sets forth three competing theories of the Cold War—the orthodox, revisionist, and critical/interpretative views. . . ." That is correct; he has made a sincere attempt to be objec-

tive about a matter on which objectivity is difficult. The book does not attempt to produce new material, but it is a clearly written assessment of the old. We may regret the brevity of the index and feel that a bibliography should have been included in the first volume, but those are small points.

As the Cold War protagonists were so obviously the United States and the Soviet Union, it is tempting for an American writer to concentrate on the debate about the Cold War within the United States and to devote his main attention to the period after those two powers were visibly engaged—roughly from the time of Yalta. As the book is about theories, it is equally tempting to begin with the theories and historiographers and only later deal with the salient facts. Thompson has done both of those things; whether he started at the right point or looked at matters in the right sequence is less certain.

The author reminds us that we may date the origin of the Cold War as far back as 1917. If we are to take a main starting point somewhere in the 1940s, it is not really appropriate to begin in 1943, skim rather lightly over the next two years, and then home in on Yalta. Long before 1945 the lines of the eventual Cold War had been largely drawn. In the summer of 1941 there were deep arguments between Russia and Poland, even though both countries were fighting the same enemy. In December of the same year British Foreign Secretary Anthony Eden was invited by Stalin to sign two treaties with a secret protocol making nonsense of the Atlantic Charter. Throughout the ensuing three years Britain was maneuvered backward until the "Tolstoy" agreement of late 1944 ended with a definition of spheres of influence in southeastern Europe. With the agreements over areas of occupation, involving the United States as well, the iron curtain was more or less drawn in Europe.

Thompson could perhaps have examined the situation after Yalta as it must have appeared from a Marxist standpoint. Granted the chaotic condition of Europe and Roosevelt's declaration that the Americans would pull out within a couple years, Stalin and his associates had every reason to think that, with minimum Soviet assistance, communist governments could be set up throughout Europe within a short time. They were probably much more interested in Germany and France than in Poland and Czechoslovakia. Yet in 1947 the Truman Doctrine and what is wrongly called the Marshall Plan made it plain that Russia could advance no farther in Europe without full-scale war; in 1950 the Korean conflict imposed similar limits in northeast Asia. Surely the key question of the Cold War was whether such limits should be drawn—and where.

ROY DOUGLAS
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ANCIENT

MARY R. LEFKOWITZ. *Heroines and Hysterics*. New York: St. Martin's Press. 1981. Pp. ix, 96. \$17.95.

JUDITH OCHSHORN. *The Female Experience and the Nature of the Divine*. Bloomington: Indiana University Press. 1981. Pp. xvii, 269. \$17.50.

Future historians will observe that feminist scholars of our generation contributed an important chapter to the history of ideas. Both Mary R. Lefkowitz and Judith Ochshorn ask new questions of well-known texts from antiquity and offer original and provocative answers.

Lefkowitz's work demonstrates that the study of women has become a respectable subject in the field of classics during the past ten years. A classicist whose expertise is in Greek poetry, Lefkowitz was the first to analyze one of the favorite topics of the feminist classicist—the poetry of Sappho. She presented her views at a meeting of the American Philological Association in 1972. This book includes the paper on Sappho, as well as other lectures, articles, and book reviews from the past ten years. Four of the sixteen chapters are previously unpublished. Notable among the new pieces is "The Wandering Womb," an essay on the politics of gynecology, exploring Greek medical fictions about hysteria and their modern legacy. In "Women's Heroism" Lefkowitz finds that unlike heroes, the heroines of epic and tragedy are usually self-sacrificing victims who acquire knowledge passively and manage to survive catastrophe. Lefkowitz's analyses are topical and generic rather than chronological. Greek epitaphs from the fourth century B.C. are discussed in tandem with Latin epitaphs from the empire. This arrangement emphasizes recurrent themes in women's lives: one that emerges is the importance of all-female groups in education and ritual from archaic Greece to modern America. Historians who study Greek women look forward to the publication of material from Brauron, in Attica, where girls dressed as bears performed rituals in honor of Artemis. Lefkowitz argues that since the girls who participated in the Brauronia were under ten years old the rite was concerned with fertility in general rather than puberty (p. 18, n. 27). The celebration was quinquennial, however. Therefore the ten-year-old "little bear" would have reached menarche at the time of the next celebration.

Lefkowitz compares her experiences as a visiting professor at Berkeley, where she was the only female member of a large classics department, to being a student and faculty member at single-sex institutions like Brearley and Wellesley. Although she cautions against using traditions about ancient authors' lives to interpret their work, it is clear that

Lefkowitz's own experiences have given her unique insights into the Greek world and its female *mileus*.

Bibliography has been added since the original publication of some of the pieces, but the book has not been thoughtfully edited. Citations of sources are often not to primary texts but to their translations in M. Lefkowitz and M. Fant *Women's Life in Greece and Rome* (1981), without page numbers. One footnote is missing (p. 28): the epitaph of Allia Potestas, a concubine praised for her cleanliness, can be found in *CIL* 6, 37965.

Judith Ochshorn, a historian who now directs a women's studies program, compares pagan religions with Judaeo-Christian monotheism in terms of the gender roles of their divinities and votaries. She argues convincingly that in ancient polytheistic Near Eastern societies the power of divinities was not distributed according to sex. Major goddesses and gods enjoyed attributes and prerogatives that were qualitatively and quantitatively the same. The sexuality and maternity of goddesses were not viewed as impure. As religious personnel, mortal women and men were equally important. A new study published in *Reflections of Women in Antiquity* (1981) supports Ochshorn's thesis. Jon-Christian Billigmeier and Judy A. Turner found that priestesses and other sacerdotal women in Mycenaean Greece were as prominent as their male counterparts.

According to Diodorus Siculus (I.27.2), because of Isis, the Egyptian queen enjoyed greater power and honor than the king, and among commoners the wife ruled the husband—the husbands agreeing in the marriage contract that in all matters they would obey their wives. Ochshorn is more hesitant than Diodorus about relating secular sex roles to conceptions of the power of male and female divinities respectively. But when she turns to Judaism and Christianity she posits a necessary connection between the prominence of the male divinity and the societal view of women as unclean, lacking morality, valuable only for their reproductive capacity, and incapable of holding religious offices.

Ochshorn ranges through traditions from Egypt, Sumer, Crete, Greece, Rome, Judaism, and Christianity. In a work of this sweep mistakes like giving the wrong dates of the Archaic Period (p. 73) may occur. Her notion that participation in polytheistic cults was not rigidly defined by sex—with priests serving gods and priestesses attached to the cults of goddesses—cannot be substantiated for Greece. Abundant written documentation argues to the contrary. The Pythia through whom Apollo spoke at Delphi is not an example of a woman in service of a male divinity, for she was a legacy of the goddess who previously occupied the site. Elizabeth Sinclair Holderman in *A Study of the Greek Priestess* (1913), testing a hypothesis formulated by L. Farnell in

1904, found that not only was a priestess more likely to serve a goddess and a priest a god, but that sacrificial victims were of the same sex as the divinity.

In using material evidence from prehistoric sites like Çatal Hüyük, the historian has little choice but to rely on the publications of archaeologists like James Mellaart. But, as the notes reveal, Ochshorn has had to rely on English translations of all the primary sources, although she attempts to discuss Phyllis Tribble's view of the masculine bias of Old Testament translations. The bibliography is sparse, with only one secondary work cited in a modern foreign language. Although in recent years there has been more progress made in our knowledge of the Bronze Age than in any other area of ancient history, Ochshorn often cites the older literature. The lack of painstaking scholarship to buttress an attractive theory is regrettable, for it will doubtless discourage historians from giving Ochshorn's book the consideration it merits.

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JEAN ROUGÉ. *Ships and Fleets of the Ancient Mediterranean*. Translated by SUSAN FRAZER. Middletown, Conn.: Wesleyan University Press; distributed by Columbia University Press, New York. 1981. Pp. 228. \$15.95.

Given the great importance of seafaring for Rome and its absolutely vital importance for many Greek cities, the poverty of our narrative sources on the ships, sailors, and navigation of antiquity is an interesting question in itself. Part of the answer, it seems, is that the seafaring milieu was a world unto itself, with its own laws, religion, and culture—which unfortunately seems to have lacked a literary aspect. Since wooden ships will not survive the centuries, marine archaeology tells us much about cargoes—and even more about containers (there is a steady crop of amphorae finds)—but very little about the ships and of course almost nothing about their sailors and the whole business of outfitting, and sailing (or rowing) ships. Inscriptions too, while not rare, are surprisingly unrevealing, and the great mass of models, sculptures, mosaics, coins, and paintings that depicts ships does not suffice to resolve many uncertainties about their structure and true form. The *quinqueremes* (or *penteres*) for example, were they propelled by five banks of superimposed rowers or five rowers alongside—both very hard to imagine—or some combination of the two? And what of the “tens,” “twenties,” and Ptolemy IV's “forty”? We can count the masts, we can sometimes guess the length, but we cannot determine much more from any of the representational evidence.

Ever since 1971, Lionel Casson's *Ships and Seaman-ship in the Ancient World* has been the authoritative work on the subject. It remains the most complete sourcebook in any language, and Casson's opinions on particular matters (including the above) are always sensible and indeed the most persuasive. Jean Rouge's much shorter overview will not displace Casson's work but it has great merit: comprehensive and systematic, but not systematically detailed. Rouge's book provides an elegant survey of the entire subject from the construction of ships (hulls, rigging, gear) to their use for trade, piracy, and war; when points of scholarly controversy are reached, the sweeping survey gives way to incisive discussion in detail, which is concluded in each case with a firm judgment almost always persuasive. As a result of its brevity and soundness, Rouge's book should attract those nonspecialist students and general readers who might find Casson's work excessively detailed.

Rouge's book covers the entire period from Minoan Crete through the *Notitia Dignitatum* of the late Roman Empire—if one may indeed use that verb to describe abrupt transitions from one glimpse to another, which is all that our fragmentary sources will allow. And Rouge makes a special effort to explore what might be called the sociology of seafaring, who did what, and why. Here too Rouge is persuasive but perhaps he should have stressed more strongly the one central fact about life on the sea: it was hard, dangerous, and sordid for all who sailed, and a risky business for those who had others sail for them, owners, traders, or rulers. Accordingly, the seafarers were largely an underclass of their station, with sailors below farmers and ship traders below land traders of equal wealth.

Rouge writes that Hesiod's famous injunction was obsolete by the time it was uttered, but it seems that the Greeks (and Romans) continued to feel as Hesiod felt, even when they did have to sail much more—and if one sails with them nowadays among their Aegean islands, one will encounter still the same attitude: the sea must be used but it is not loved. This is of course one reason for the poverty of our narrative sources: most of what we have consists of complaints against ships, sailors, captains, and—above all—the sea.

An excellent translation and beautiful book-production enhance the appeal of this edition of an attractive work.

EDWARD N. LUTTWAK
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G. E. M. DE STE. CROIX. *The Class Struggle in the Ancient Greek World: From the Archaic Age to the Arab Conquests*. Ithaca: Cornell University Press. 1981. Pp. xi, 732. \$49.50.

G. E. M. de Sainte Croix's long-awaited book attempts a revolution in ancient historical studies. Both theoretical and practical, it first sets forth an understanding of Marxism and then seeks to demonstrate Marxism's usefulness for the systematic analysis of Greek (and, perforce, imperial Roman) history. It also in effect extends Marxist analysis itself, insofar as Marx never provided a complete or coherent exposition of classical antiquity.

De Sainte Croix rightly claims he has not committed an anachronism by applying to a precapitalist society an interpretative model developed with a view to nineteenth-century industrialism. After all, Marx recognized and stressed the essential differences among various economic forms of society. This in itself, however, in no way undermines the validity of the assertion that "class," the collective social expression of exploitation embodied in the relationship of a group of individuals to the conditions of production and to other groups, is the most important key to the understanding of all societies. Moreover, the author feels that it is precisely these shared features of class and its corollaries, exploitation and struggle, that make the study of the ancient world relevant to the modern.

He also claims that his approach is the truest way to study ancient history because it alone permits analysis and explanation of change; and history is change. His Marxist critique does not merely describe given periods, frozen moments; and it is systematic and encompassing, studying a whole society and not just parts of it. It follows that he rejects the prosopographical method because it is too concerned with elites and too little appreciative of the necessary effect of the masses on the minds and deeds of leaders. He condemns with justice the antiquarian fact-gathering school for often being blind to its own biases and for being incapable of providing a systematic presentation of its subjects of study. He has little sympathy, either, for those historians who see in social facets other than class, like status and juridical orders, the clue to clarifying the secrets of ancient history. Such an approach, while it can enrich our understanding, is inevitably static and can never fulfill the need for an analysis of change over time.

De Sainte Croix's rejection of the validity of all other approaches recalls Marx's. One of the least appealing characteristics of his book is his intolerance not only for other scholars but also for all social forces that are objectionable in one way or another. Too often criticism sours into snideness, irony into sarcasm.

The actual explication of the movement of Greek history builds from the premise that classical antiquity was a slave economy. It was through the exploitation of slaves that the dominant propertied class achieved a surplus from its control of land, the

means of production. This was the best available way for the dominant class to free itself from having to work, given the technologically and economically poor state of farming in antiquity and the small numbers of free laborers that could be hired.

Oligarchies extended exploitation to the free but poorer citizenry, while democracies helped to check this. The Roman conquest, however, made an end of democracy, and the Roman peace led to a greater squeeze on the free poor, since no war meant no captives and a corresponding drop in the supply of slaves. The rigidifying of late antique society and the increasing harshness of the legal system on the lower classes and even the *decurions* reflect the economic drive of the dominant class to preserve its surplus—and its privilege. This development only worsened the situation, however, which an increasingly large imperial administration and the Christian church further exacerbated with more and more nonproducing bureaucrats and clergy. Finally, there were few to fight against or mourn the collapse of this society beneath the onslaught of the barbarians.

Any book of this scope is bound to invite dissent. At the level of theory, for instance, the whole notion of the labor theory of value is disputable. The perception of slaves as an exploited class strictly speaking is also problematic, for Marx understood slaves as part of the means of production, analogous to Aristotle's definition of them as animate tools. Further refinement is needed here, for otherwise it does not seem to make sense to speak of "machines" as a class.

Yet the success of this book for most readers will probably not depend on the validity or orthodoxy of its Marxism. What most people will desire is a fresh insight into ancient life unobtainable elsewhere. Unfortunately, they will not be satisfied. Once he has established his critical categories, de Sainte Croix proceeds to a narrative that is often just a predictable rehearsal of various examples of struggle and exploitation. This is only another form of description, not analysis. His understanding of phenomena like Greek tyranny, country and city, the urban crowd, or legal developments does not really advance much over the work done by other scholars. Readers may not feel that the value they gain is worth the effort of toiling like members of an exploited class through an intolerably prolix text.

Although the book aspires to fresh innovation, it winds up being strangely stale. It does not seize the opportunity to provide a tangible sense of what production, class, surplus, and so forth, mean within the concrete context of antiquity. It does not, for example, ask or answer interesting questions like how the level of technology in this society is a function of its mode of production. All too often the blunt use of critical categories risks the charge of

reductionism, while the presentation of events turns into what remains an old-fashioned sort of narrative history, albeit with a Marxist twist.

It must be stressed, nonetheless, that we are all in de Sainte Croix's debt for understanding his approach as possible and valuable. He joins the ranks of the pioneers like Rostovtzeff and Jones in the study of the social history of antiquity. Few will match the range and thoroughness of his scholarship. One can hope that others will be able to advance further where he has led.

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WALTER EDER. *Servitus Publica: Untersuchungen zur Entstehung, Entwicklung und Funktion der öffentlichen Sklaverei in Rom*. (Forschungen zur Antiken Sklaverei, number 13.) Wiesbaden: Franz Steiner. 1980. Pp. xv, 187. DM 54.

The theme of Walter Eder's excellent monograph is narrowly defined: the group of state-owned slaves described by Roman sources as *servi publici*. This class of slaves is associated mainly with the old republican magistracies and priesthoods, not only during the late Republic but also during the early centuries of the empire. They are first attested in our sources for 312 B.C. and are frequently mentioned later.

The greater portion of Eder's monograph carefully isolates their characteristics. The first and most obvious is public ownership (pp. 6–33). Where this was not a consequence of birth, it usually resulted from capture, purchase, gift (including inheritance), or judicial judgment. The wars of conquest were perhaps the most common source. For instance, in 209 B.C., after the capture of New Carthage, Scipio Africanus singled out nearly two thousand artisans and made them *servi publici* (Livy 26.47.2). As this source and many others show, not every slave acquired by the state automatically became a *servus publicus*; public ownership was a necessary but not a sufficient condition.

Eder locates a second condition: that the slave be deliberately kept in long-term service to the state (pp. 34–101). Historians will find these pages rewarding to read. *Servi publici* appeared most frequently as the servants and helpers of the traditional priesthoods and of the Republican magistrates both at Rome and in the provinces; but they also represented the state as messengers in its relations with citizens, and occasionally even acted as firemen or policemen. Nonetheless, their jobs rarely involved policy initiatives and were instead confined to menial tasks. Priests and magistrates, even quite minor ones, were allotted their contingents of *servi publici* and could receive additional help if extraordinary

circumstances arose; the slaves were shuffled about from post to post, and there was little specialization of work.

Despite their lowly employment, *servi publici* in time acquired numerous special privileges (pp. 102–23), presumably by virtue of their direct connection with the state. They wore special clothing, undertook “marriages” with freed or even free women, were given legal power to dispose by will of half their *peculia*, and were virtually guaranteed manumission as a reward for loyal service. In short, they emerged as a privileged caste of slaves, their status directly foreshadowing the rise of the *familia Caesaris* in the empire.

The second and shorter portion of Eder’s monograph deals with the origin and development of public slavery (pp. 126–71). Eder speculates that the institution began in the first half of the fourth century B.C., primarily as a result of pressing military needs and the passage of the state from a patrician-gentile model to a more embracing and civic structure. The subsequent failure of the *servi publici* to evolve into a protobureaucracy is largely related to the republican aristocracy’s similar failure to transcend its original urban character. In the empire both institutions suffered slow but certain eclipse.

The concentration and clarity of Eder’s study are attributable to the influence of Joseph Vogt, whose “antiquarian” approach to slavery has recently been subjected to scathing attack by Moses Finley. Yet Eder’s monograph yields so many valuable insights into the structure of ancient society that I would be highly reluctant to see such studies cease; and the race, thank God, is not always to the swift.

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INGEMAR KÖNIG. *Die gallischen Usurpatoren von Postumus bis Tetricus*. (Vestigia, Beiträge zur Alten Geschichte, number 31.) Munich: C. H. Beck’sche Verlagsbuchhandlung. 1981. Pp. xiii, 237. DM 68.

From A.D. 260 to 274, Gaul, Germany, Britain, and Spain constituted a separate empire, divorced from the emperors at Rome. Here is an attempt to write the history of the rulers of this *imperium Galliarum* and their relations with the central power. The chapters of this monograph are appropriately named after the successive “usurpers”: Postumus, Marius, Victorinus, and Tetricus.

The restriction to a mere history of the rulers was necessary since the sources are exceedingly scarce for this period and will not support yet a full history of the time in all its aspects. Our most extensive sources are coins revealing the program, propagan-

da, and residences of the rulers; inscriptions documenting their areas of domination; and the highly treacherous *Historia Augusta* providing colorful if untrustworthy portraits of the characters and lives of these rulers.

Ingemar König brings together all these sources and a very extensive literature for a comprehensive and balanced treatment. In clear and enjoyable prose he guides the reader through the events from the rebellion against Gallienus to the reconquest by Aurelianus, rightly stressing the fact that there existed no Gallic separatism but merely a separate defense against the incoming waves of Germanic raiders and settlers.

Alas, where reliable information is scarce, speculation prevails. Previous research on these reigns, as outlined by König, constitutes an *irrgarten* of such exasperating contradictions that even the most undaunted will get weary. And the weariness is not overcome by the author’s own suggestions. While König is certainly right in exposing many false theories, his own conclusions are often likewise unconvincing if not demonstrably wrong. Among his methodical shortcomings is an eagerness to discuss and weigh the motivations supplied by the *Historia Augusta* for the pronunciamientos and acts of the usurpers, while actually these motivations, fictitious as they are, have no information value at all. Such overconfidence in the *Historia Augusta* is matched by frequent efforts to extract more from the inscriptions than they will really yield.

One is grateful to the author for having appended a collection of all inscriptions referring to the usurpers of the *imperium Galliarum*. The fact that they are so few and so unevenly distributed shows that the many efforts to use epigraphical statistics here are entirely futile and will not lead to reliable results. Several texts demand a different interpretation. Thus the dedication from Vindonissa mentioning a *praeses* of Upper Germany in A.D. 260 does not constitute proof that Gallienus had, by then, taken away the governorships of military provinces from the senators and given them to men of equestrian rank, for H. G. Pflaum (*Les procurateurs équestres*, [1950] p. 112) has shown that the term *praeses* applied equally to senatorial and equestrian governors. As a consequence, one cannot say that the usurpers “continued the reforms of Gallienus,” or that there is now added reason to believe Postumus was a governor (that is, equestrian *praeses*) when he rebelled. Similarly, the title *Tetricianorum* of an auxiliary regiment in Britain does not imply Britain recognized both Tetricus senior and Tetricus junior: the plural merely refers to the soldiers in the unit.

The value of this study, then, consists not so much in new insights as in bringing together most of the literature on the period and in discussing most of

the questions posed by our knowledge of these reigns.

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TIMOTHY D. BARNES. *Constantine and Eusebius*. Cambridge: Harvard University Press. 1981. Pp. vi, 458. \$35.00.

This is an erudite and critical rereading and original reinterpretation of the sources on Constantine I and Eusebius of Caesarea, the result of much reflection on these difficult, important, and much-studied subjects. The emphasis is, as the title indicates, on Constantine and Eusebius, on political and ecclesiastical history, not on the institutional, provincial, social, economic, or broader cultural history of the age of Constantine. The new contributions are found in its chronology and many specific details, not in any broad thesis or conceptual framework. It is, in fact, really a re-examination of the chronological, and to some extent prosopographical, foundations of the history and sources of the late third and early fourth century, not just the lives of Constantine and Eusebius. Yet it is impossible to make an accurate evaluation of the arguments about chronology without access to the accompanying yet still, at the time of the writing of this review, unpublished volume on *The New Empire of Diocletian and Constantine*, which contains lengthier and more detailed investigations and proofs, to which the reader of the footnotes is repeatedly referred. It is regrettable that the two volumes were not simultaneously published. *Constantine and Eusebius* does contain learned notes, but there are many important cross-references to *New Empire*, which this reviewer cannot evaluate at this time.

The exposition is concise, compressed, and reads smoothly. Yet Timothy D. Barnes attempts (p. v) "first to delineate an accurate portrait of each man separately, then to depict their relationship to each other." In part 3 Barnes makes a strong and very important case for the relative lack of frequent contact between Eusebius and Constantine during Constantine's lifetime (pp. 265–68). These negative conclusions reinforce the two separate biographical portraits and result in an absence of absolute unity to the book because the two subjects remain somewhat separate.

Barnes's reconstruction of the eternal and basic issues of the conversion of Constantine, the policies of Galerius and Licinius, and Constantine's religious policy ("coherent and comprehensive" [p. 247]) is sensible. His command of patristics, ecclesiastical history, the sources, and the modern scholarship on Constantine, as well as his familiarity with the larger

historical context, is quite impressive. He admits difficulty in assessing Constantine's administrative changes. Not every scholar will agree with Barnes that "the military hierarchy came to be entirely separate from the civil" (p. 256) because there are those (R. Tomlin, among others) who argue that any notion of rigid separation requires qualification; this subject, like many others that are mentioned in this book, deserves more inquiry. Many assumptions, assertions, and conclusions are consciously controversial; for example, Barnes's analyses of Eusebius take issue with those of H. Drake, R. M. Grant, and other scholars who no doubt will explain their positions on specific aspects of Eusebian scholarship. Proofreading lapses are rare, for example, "Maximian" (*Contents*). Barnes's skeptical interpretation of the *Life of Constantine* (pp. 267–71) is prudent, irrespective of the validity of his many other technical revisions. *Constantine and Eusebius* is an indispensable although controversial book that every investigator of the late third and early fourth centuries must use, and some must confront.

WALTER EMIL KAEGI, JR.
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GERARD E. CASPARY. *Politics and Exegesis: Origen and the Two Swords*. Berkeley and Los Angeles: University of California Press. 1980. Pp. xv, 215. \$20.00.

After a brief introduction on Origen himself, Gerard E. Caspary enters upon his complicated task by discussing Origen's exegesis of Joshua's conquest of Canaan. He rightly notes that in Origen's view the biblical wars actually took place, though their only significance for Christians lies in a "spiritualized" form. Here there seems too much detail and quotation, not enough comparison of this idea with others. Why does Origen say what he says? What could have been discussed are Strabo's account of Moses' own unopposed capture of Jerusalem (xvi. 2. 36) and Philo's rejection of "history" in favor of a "rational" account of the Canaanites' voluntary surrender (Eusebius, *Praep. Ev.* viii. 6. 5). Simply to paraphrase Origen is not enough, and it is not helpful to analyze his exegesis or exegetical method just in relation to itself. The same comment must be made on chapter 2, where the "two swords" beloved of medieval exegetes are generally read into what Origen says. We are not surprised to find, in a "hermeneutical interlude" of chapter 3, that the figure of Lévi-Strauss suddenly appears but does not provide real guidance. "The dialectical pairs inserted in the Pauline circle thus allow the exegete to express the radical ambivalence of the present dispensation far better than the tropological level of the linear structure" (p. 121). The word "parameter" also rings a warning bell.

The last chapter, on Origen's theology of politics, is sensible although not altogether novel. Caspary might have done better to begin with this relatively firm foundation instead of putting the superstructure (or structuralism) up first. A minor criticism could rise concerning the use of King James. Why not put Origen in Elizabethan English too? But avoid saying, "like He" (p. 184, 127).

I am sure that Origen is subtle, but perhaps not as subtle as Caspary makes him. The idea of investigating his language from a modern viewpoint, one notes, has been tried less thoroughly by Robert W. Jensen in *The Knowledge of Things Hoped For* ([1969], pp. 24–57). Overall, I think there is too much exegesis, too little politics here. There is a tendency to burrow or wander in Origen's presumed thought world. The political comments, however, seem astute: note especially page 137, note 37, on Hippolytus' dislike of democracy. We close with a bit of trivia: the "free children of a king" idea noted on page 170 is not actually "Hellenistic romanticism" but reflects Gnostic exegesis described by Clement of Alexandria (*Stromata* iii. 30).

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SABINE G. MACCORMACK. *Art and Ceremony in Late Antiquity*. (Transformation of the Classical Heritage, number 1.) Berkeley and Los Angeles: University of California Press. 1981. Pp. xvi, 417. \$39.50.

This study is an important and imaginative piece of scholarship that sets out new directions for the study of late antiquity. Central to that period were the role of the emperor and the struggle between paganism and Christianity, and Sabine G. MacCormack skillfully traces changes in fundamental attitudes from the time of the Tetrarchy to the reign of Heraclius. She uses as her focus three critical events in the life of the emperor and of the state as a whole: *adventus* (the "arrival" of an emperor), *consecratio* (death and subsequent "divinization" of an emperor), and accession. These were all important events, at which contemporaries could express their views about the world, the gods, and the relationship between the two; they were also critical points in the practical sense that it was then that imperial power was made manifest and "real," and the subjects of the empire might express their acceptance—or rejection—of prevailing personalities and ideas.

Adventus, *consecratio*, and accession have, of course, left abundant though uneven reflections, and it is from this evidence that MacCormack begins her analysis. Thus, the prose and poetic panegyrics, written to commemorate or celebrate great imperial events, provide the primary framework for the book. Reflections of ceremony, especially as record-

ed in the *De ceremoniis* of Constantine Porphyrogenitus, provide information as to what was actually done on such occasions, while art, especially coins and ivory, occasionally mirror the same events. It is in this threefold use of evidence that the book makes its greatest contribution, for the author combines ideas and images skillfully and with enviable familiarity. In the end, she argues that the use of such analysis will help to get around one of the most troublesome problems of the premodern historian—the absence of critical data—and to draw conclusions of larger validity.

Among the book's many arguments, a few may be singled out for mention here. Thus, MacCormack suggests that the Tetrarchy was a rare period of harmony and integration when "the tension between ideal and reality in public language was reduced to a minimum" (p. 270). Constantine and his successors introduced new concepts, and consistency was never again possible within a classical framework. Another important consideration is the relationship between imperial and Christian forms and expressions, where it has long been known that the church was in large measure the borrower; MacCormack is able to point out that after the application of imperial imagery to Christ, this repertoire was no longer appropriate for secular use and the panegyrists had to turn to other sources of inspiration. Finally, MacCormack notes the increasingly urban (as opposed to military) orientation of ceremony from the fifth century onward; this fits nicely with what we know about the growing importance of Constantinople in this period and the role of the nonmilitary emperors of that age.

There are other themes and considerations, and the book will repay careful study by anyone interested in late antiquity and the early Middle Ages. Unfortunately, the book is not always well written and it is definitely too long: a careful editor might have cut the length by half and made a better book. Frequently the argument is confused by the attempt at elegant language and a wordiness that would have made the antique panegyrists proud (including sentences up to nine lines in length). In addition, a few obvious considerations, such as the role of magic and the development of Christian ritual, were left out of the account, presumably because they were not discussed by the panegyrists. Finally, there are some substantive problems. Thus, it is unlikely that the condemnation of certain emperors in the Roman historiographic tradition "was based on their attempted self-exaltation to a divine level" (p. 103). The *vota* issues (pp. 165 and following) and the symbolism of the torque (pp. 241 and following) are not discussed in a satisfactory manner, and the nonnarrative aspect of late antique art (p. 275) is only mentioned. These defects are, however, minimal, and this book will certainly remain an impor-

tant source of stimulation and discussion for some time to come.

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KENNETH C. GUTWEIN. *Third Palestine: A Regional Study in Byzantine Urbanization*. Washington: University Press of America. 1981. Pp. xiv, 416. Cloth \$25.75, paper \$15.50.

This is the first fully documented history of the Byzantine province of Third Palestine. It is based on classical, Byzantine, and patristic literary sources. Also excellent use is made of archaeological sources, especially the 1935 Colt Archaeological Expedition that excavated the hitherto forgotten city of Nessana and found there, among other artifacts, over 200 papyri. The archaeological sources also include the results of post-World War II excavations at other sites in *Palestina Tertia* including the towns of Oboda, Kurnub, Elusa, Ruheiba, and Tamara.

Kenneth C. Gutwein shows that during the Byzantine period this rather barren area had its greatest population, most extensive trade, and most intensive system of irrigation and cultivation. Gutwein examines the development of urbanization in the province from the period A.D. 300–636 and clarifies many problems of town planning, architecture, and the relationship of the towns to the countryside. The many diagrams of town plans and the ground plans of buildings are especially noteworthy.

The first of the six chapters of this work deals with the geographical limits and the history of *Palestina Tertia* from about A.D. 300 to 636. The second chapter, containing helpful charts and graphs, gives an idea of the soil, topography, and climate of the province, and the third chapter deals with its historical geography. The fourth chapter, concerning the towns of the province, contains valuable plans of the urban sites, of urban church architecture, domestic architecture, and the public and commercial quarters of the towns. The work is concluded with chapters on the commercial and military roles of Third Palestine.

From his study the author concludes that the original impetus to urbanization and population growth occurred because of the strategic location of the province for the military and because of Third Palestine's topography and hydrology. Then as the military significance of the province began to wane in the early years of the Byzantine era because of reliance on the phylarchate, a second wave of prosperity came from the bands of Christian pilgrims on their way to holy places such as the Monastery of St. Catherine in the Sinai. Arab raids began in the early seventh century, and the province was lost to the empire after 636.

As a pioneer work on the subject this volume is important and contains a wealth of information, but it could be much improved. The maps, which appear to be hand drawn, should be much more informative, and the Madaba map, often mentioned, should be included. The work abounds in errors such as in the spelling of English as well as Latin words, lack of agreement of subject and predicate, and other such problems that should have been eliminated by a careful proofreading.

While not easy going for the general reader, this study will provide much valuable information for students of late antiquity and the Byzantine empire. One might hope, however, that if another edition is contemplated, Gutwein will improve on the format of the book and the proofreading of its contents.

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MEDIEVAL

JACQUES LE GOFF. *La naissance du Purgatoire*. (Bibliothèque des Histoires.) Paris: Gallimard. 1981. Pp. 509.

As Dante in emerging from his *Purgatorio* received "crown and mitre," so Jacques Le Goff deserves a historian's crown for his stimulating survey of the growth of a thicket of theological and folkloric beliefs that flowered most luxuriantly in Dante's second canticle. Le Goff's major accomplishment is to demonstrate that Purgatory was not "born" until the twelfth century, if one understands by Purgatory a "third place" between hell and heaven meant for the ultimate expiation of sin. After reviewing conceptions of life after death in pre-Christian religions and the Bible, Le Goff shows that the "Fathers of Purgatory" (pun intended) were two Greeks, Clement of Alexandria and Origen, and two Latins, Augustine and Gregory the Great: the former were the first to teach the doctrine of a transcendental purifying fire and the latter specified that this fire would cleanse a class of middling sinners in the time between their individual deaths and the general judgment. But there was no Purgatory localized as a third place until theology merged with folklore to produce a new belief system in the twelfth century. Afterwards, the concept of the third place matured quickly under the care of Scholastic theologians and purveyors of *exempla* until it reached its majority, conveniently enough, in 1300, the first jubilee year and the fictional date of Dante's otherworldly journey.

No serious reader of this wide-ranging account will fail to be thoroughly engaged by it, seeing things that he or she has never seen before and disputing

mentally with an author who prefers to be categorical and provocative. As for myself, I was made to recognize interrelations between conceptions of transcendental resting or purgatorial states and this-worldly chiliastic beliefs. Above all, I was impressed at how Le Goff's main arguments enhance the current view of the High Middle Ages as a time both of humane optimism and of rapidly growing pretensions on the part of the Roman church. But I also wonder whether Le Goff is correct in maintaining that use of the noun Purgatory first occurred in the decade between 1170 and 1180 rather than several decades earlier (his technical scholarship in rejecting alternatives leaves much to be desired). More importantly, I wonder whether he does not overstate his case by insisting on a quick "delivery" of Purgatory primarily verified by the appearance of a noun. For example, it seems to me nearly impossible to account for the success of the Cluniacs without conceding their role as intercessors for suffering souls (a subject Le Goff downplays) or to account for the appeal of early crusading indulgences without recognizing that the thousands who endangered their lives to go crusading must have thought that they would thereby be gaining remission of transcendental—ergo purgatorial—punishments (a subject he does not cover at all). In other words, granted the interest and significance of asking when Purgatory was "first envisaged" as a place, it seems to me at least as important to ask when purgatorial punishment began to make a difference as a state that might be mitigated by the prayers of monks or avoided by the fiat of popes, and I suspect that pursuit of this question would take some of the spotlight off of Le Goff's favored twelfth century. Still, there can be no doubt (with apologies to T. S. Eliot) that "There was a Birth, certainly," and this book surely brings us close to it, "down to a temperate valley, wet, below the snow line, smelling of vegetation."

ROBERT E. LERNER
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PETER HERDE. *Cölestin V., 1294 (Peter vom Morrone): Der Engelpapst; Mit einem Urkundenanhang und Edition zweier Viten.* (Päpste und Papsttum, number 16.) Stuttgart: Anton Hiersemann. 1981. Pp. xii, 447. DM 220.

Peter Herde is known to historians for his studies of the papal chancery and a groundbreaking work on the *Audientia litterarum contradictarum*. The high standard of these books is equally met in this new biography of Pope Celestine V (ca. 1210–95). The first four chapters deal in chronological order with the life of Peter, who was probably born in S. Angelo Limosano and is called "of the Morrone"

because most of his life was spent on the Monte Morrone near Sulmona. They are devoted to his early life, the events preceding and surrounding his election to the papal see, his brief pontificate as Celestine V, his resignation, his foiled attempt to return to the life of a hermit, and his death. Two concluding chapters contain an excellent survey of the contemporaneous canonical and polemical literature concerning the validity of Celestine's resignation, a review of the events leading up to his canonization, and, finally, a look at the notion of the angelic pope as it applies to Celestine V. A substantial appendix, comprising almost a third of the entire book, will be particularly appreciated. It contains ten hitherto unpublished or badly published documents, among which Celestine's confirmation of the agreement between Charles II of Anjou and James II of Aragon of December 1293 at La Junquera deserves special mention. In addition, there is a previously unedited, anonymous Latin vita of Celestine V, written between 1327 and ca. 1340, and a vita in the vernacular of Bergamo, written and probably composed by one Stefano Tiraboschi in the first half of the fifteenth century. The book concludes with a bibliography and three excellent indexes to the narrative section and the appendix.

The strange story of the octogenarian hermit of peasant origin who was called from his cell in the mountains of southern Italy to become the ruler of Latin Christendom has been told before, notably by G. Celidonio and F. Baethgen. It may therefore come as no surprise that Herde has no startling revelations to present. There are new emphases to be sure. He underlines, for example, that Peter of the Morrone had much administrative experience as the head of his new order, and he points out that in 1294 Celestine V may have been not so much naive as simply old. But even so he agrees with the conventional judgment that Celestine V was clearly incapable of functioning as pope. The value of Herde's book must thus be seen in the precise detail with which he has managed to fill an otherwise familiar picture. He settles scores of minor points of controversy, gives the first complete analysis of the electoral college of cardinals and their motivations in 1294, and provides a useful survey of the curial personnel under Celestine V and of the cardinals created by him. There can be little doubt that this will be the standard treatment of the subject for many years to come.

CONSTANTIN FASOLT
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JANET COLEMAN. *Medieval Readers and Writers, 1350–1400*. New York: Columbia University Press. 1981. Pp. 337. \$27.50.

This book is part of a series whose aim is to examine the role of English literature in history. It is the author's view that the period 1350–1400 was one that witnessed a remarkable growth of lay literacy and social mobility. This fourteenth-century literature had, moreover, significant “political and socioeconomic implications” that encouraged “critique and change” (p. 17).

Janet Coleman divides her interesting and provocative essay into four major topics. She turns first to a survey of vernacular literacy and lay education that were flourishing in the latter part of the fourteenth century. The use of English in poetry and prose was becoming more widespread. Grammar schools were proliferating and more opportunities were being created at Oxford and Cambridge for the study of logic and philosophy. Fourteenth-century civil service bishops had quite extensive academic training, and just behind them were lawyers and physicians who were the sons of the urban “middle class.” More people were reading for pleasure and more books were being written for “spiritual edification and social reform” (p. 42). This increase in the number of medieval writers and readers had important repercussions in effecting change in a society that was becoming increasingly “urbanized” and more aware that “unfreedom and villeinage had become an outrage” (p. 48).

Coleman then turns to an examination of the literature of social unrest. Here she demonstrates how this literature, primarily a product of “a newly literate and newly vocal” (p. 64) middle class, articulated many of the complaints of the age, and reflected attitudes toward church hierarchy and practices as well as attitudes toward the secular feudal hierarchy and local and national government. Drawing widely on Langland's *Piers Plowman*, Chaucer, and John Gower, the author takes up in detail many kinds of complaints, documenting these with literary evidence.

In the next chapter, Coleman attempts to draw some conclusions about the nature of preaching and the gradual decline of memory in favor of the written text. The proliferation of preaching handbooks and dictionaries of *exempla* as well as the demand for the Bible in the vernacular were products of a “widespread alphabetic culture” (p. 159). Lollard writings from about 1384 to 1425 reflected “the wider literary trends of the period” (p. 212). Whether they were sermon cycles, Bible translations, or political tracts, these Lollard works had primarily a didactic purpose.

Finally, the author addresses the way in which the ideas of Scholastic theology filtered down to non-Scholastic literature and poetry. The non-Scholastic literature that was available to the literate in Latin and increasingly in English took up and discussed political and theological themes that would have

been limited a century earlier to a university and clerical elite. Expanding economic freedom and power raised the consciousness of a more widely literate society to both social responsibility for the common good and to private responsibility for one's own salvation.

The implications of Coleman's thesis are explosive, and one would have to concur that the pen can indeed be more powerful than the sword in effecting profound social change.

PHYLLIS B. ROBERTS
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CHARLES ROSS. *Richard III*. (English Monarch Series.) Berkeley and Los Angeles: University of California Press. 1981. Pp. liii, 265. \$24.50.

As all teachers of English history know, the subject of Richard III rarely fails to arouse student attention, indeed, their passions as well. This new volume in the current “English Monarch Series” will therefore be welcomed at the pedagogical level and very likely will be accorded a good reception among professional historians also.

Charles Ross's treatment is largely a chronological and political one, but before he embarks on his main task he provides in his introduction an illuminating analysis of the sources for the reign and a historiography of the subject. Both here and later in the book he offers balanced assessments of the work of all the contributors to the Richard III debate and rarely gives them, even the extremely partisan, less than their due.

Because so much of Richard's life was spent serving his elder brother Edward IV, there has to be a considerable part of the book devoted to the first Yorkist reign, although for closer detail the reader is frequently referred to Ross's earlier volume on Edward IV. This is probably the best that can be done, but it shows the difficulties inherent in the study of English history by reigns.

The tenor of Ross's main argument, and it is an argument that is well supported, is that Richard was disliked in his own time and thought by contemporaries capable of virtually any kind of mischief. As to the notorious matter of the king's nephews, Ross argues convincingly what a number of historians have been saying in recent years, namely that Richard was indeed their murderer. In the skills of kingship Richard was competent but no paragon. He successfully widened his base of support in 1484–85 and only lost at Bosworth because much wealth and power had become concentrated in the hands of a very few magnates, two of whom proved to be disloyal.

Ross is at his best when he pauses to analyze the aspirations, affinities, and political attitudes of various members of the upper classes. The only weakness here to my mind is a failure to demonstrate what exactly the essence of social obligation was at this time and how it affected the ruling of the realm.

Only a relatively small amount of space is given to the separate treatment of specific themes, such as government, which reflects no doubt the shortness of the reign and the lack of research in these areas by others. Ross's touch in regard to government is much less sure. Matters of law are not encapsulated as neatly as they might be, nor set in their proper legal and social context. Legislation, for example, needs to be placed in a setting far wider than the actual reign or those immediately preceding and succeeding. There is no mention of royal policy in regard to the privileges of the church (benefit of clergy), nor does the regulation of commerce, as, for example, through penal laws, get any attention.

The writer has a tendency to reiterate at times and there are a number of misspellings and small errors of fact. The index is disappointing; topics (*rerum*) are only listed under Richard's name and in a way that defies logic. In sum, this is a useful and readable book, but one with a number of blemishes.

J. G. BELLAMY
Carleton University

CHRIS WICKHAM. *Early Medieval Italy: Central Power and Local Society, 400–1000*. (New Studies in Medieval History.) Totowa, N.J.: Barnes and Noble Books. 1981. Pp. xi, 238. \$26.50.

Chris Wickham's work is an overview of the history of northern and central Italy from the late Roman to the precommunal period. In presenting this overview, Wickham pays particular attention to the devolution of authority from the rulers of the kingdom of Lombardy with its capital at Pavia to regional and local power-holders—counts, margraves, bishops, and cities. Hence, his dual title. The work deals briefly with the late Roman and Ostrogothic efforts to govern Italy as an entity and then turns in detail to the building of the Lombard kingdom in northern and central Italy. He carefully weighs modern scholarly statements on the Lombards, as conjectural as many of them will always be.

Wickham is sensitive to the changes in society, to the structure of control on the land and in the cities, and to the relations between country and town from the beginning of the Lombard period on. His discussions of these themes reflect the insights gained by current Italian scholars such as Tabacco, Violante, and Fumagalli. By the middle of the eleventh century, the power of the originally very effective Lombard kings had waned, and the cities

became more important centers whose authority was exercised through the bonds of landholding and vassalage. Wickham pays particular attention to the middle people in this changing society, applying and qualifying the findings of Tabacco on the royal freemen or *arimanni*. He does not make use of Hagen Keller's *Adelsherrschaft und städtische Gesellschaft in Oberitalien, 9. bis 12. Jahrhundert* (1979). Keller's penetrating analysis of the upper and middle nobility, the *capitanei* and the *valvassores*, would have allowed Wickham to refine his statements on the nobility's share in the growth of civic government. He makes good use in this respect of an earlier study, Hansmartin Schwarzmaier's *Lucca und das Reich* (1972). Generally, Wickham's work would benefit from a sharpening of definitions and a reduction of the occasional sociological vocabulary, which the poor documentation of this period does not support.

The book is particularly useful because of its separate English- and foreign-language bibliographies, a good starting point for the undergraduate student of early Italian history as well as for some graduate students. The author states in his introduction that he devotes only very limited attention to the history of the church and to southern Italy. These omissions, however, mean more than a limited coverage: not dealing with the beginnings of the Patrimony of St. Peter and of papal government or with the Byzantine south adequately prevents Wickham from developing an understanding of Italy in its Mediterranean and European context.

Early Medieval Italy is followed, in the program of the publisher, by J. K. Hyde's *Society and Politics in Medieval Italy: The Evolution of the Civil Life, 1000–1350*. This sequence makes available to the student an introduction to the history of northern and central Italy from the end of Roman government to the beginning of the Renaissance.

REINHOLD SCHUMANN
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JONATHAN B. RIESS. *Political Ideals in Medieval Italian Art: The Frescoes in the Palazzo dei Priori, Perugia (1297)*. (Studies in the Fine Arts: Iconography, number 1.) Ann Arbor, Mich.: UMI Research Press. 1981. Pp. xii, 187. \$34.95.

LINDA SEIDEL. *Songs of Glory: The Romanesque Façades of Aquitaine*. Chicago: University of Chicago Press. 1981. Pp. x, 220. \$25.00.

Two recent art historical studies offer new insights on societal changes in the High Middle Ages by viewing familiar medieval monuments in a new context. *Songs of Glory* discusses a large group of twelfth-century Romanesque churches in Aquitaine

that display distinctive similarities both in decorative elements and iconography. The Aquitainian group can be distinguished from other regional types by the repeated use of arches over the portals and across the upper stories of the west wall. In some of the more elaborate variants, turrets supported by bundled shafts flank the mural surface. The arches above the doorways and upper areas of many façades contain sculptural decoration depicting biblical and nonbiblical subjects.

Linda Seidel attempts to offer an explanation for both the architectural elements and the figural scenes. The arches and towers are based ultimately on Roman triumphal forms that were incorporated into the design of liturgical objects during the Carolingian period. The Carolingian objects employed the arch as a reference to the spiritual victory available through the Eucharist, a meaning that Seidel believes was transferred directly from these liturgical objects to the Aquitainian façade. The theme of triumph is also identified with the figural scenes where traditional Christological representations signify the victory over death. Beside these scenes others appear that emphasize the struggle of one group to achieve this celestial reward. These representations include a number of equestrian figures that Seidel associates with a rising group of lords who exercised considerable influence in this region of France. Both their symbols of secular power and the special means by which they hoped to achieve salvation through the Crusades are identified with a number of unusual iconographic motifs. The great strength of this study lies in identifying the façades with the aspirations of this new group of patrons and elucidating the ways in which the imagery was developed to express their concerns and position. Particularly illuminating is the discussion of the influence of the Muslim princely class who were regarded as peers. The contact may not only have supplied specific visual forms but, ironically, the Islamic concept of holy war.

Less successful is the discussion of the source and meaning of the architectural elements that decorate the entrance walls. In contrast to the broadly based consideration of the figural scenes, the explanation is limited to a single source. This is especially puzzling in view of the vast range of possibilities. Her comparisons for the bundled tower-like elements are interesting and may form a partial explanation, either directly, as she suggests, or indirectly, as a reference to an architectural source. Neither the derivation of the arch form from Carolingian liturgical objects nor the relationship of this Carolingian group to Roman triumphal forms is entirely convincing. Seidel offers few Carolingian precedents and they are for the most part drawings and paintings of lost works, which may account for the lack of striking visual similarities to the Aquitainian façades. The relationship of these Carolingian objects to

Roman art is also extremely problematic. She cannot prove a connection between a Carolingian example and the proposed prototype, a Roman imperial funeral pyre. Nor does she convincingly isolate a specifically triumphal meaning for the arch on the Lorsch book cover.

Considering the widespread use of the arch, one wonders whether it is possible to isolate a source for the Aquitainian arcades. Were such a thing possible, architectural precedents for the architectural application of the form should certainly have been explored. Seidel does allude to such sources elsewhere in the book. In discussing the angels at Angoulême, she cites parallels to Roman monumental forms still accessible in southern Europe. Such monuments provided precedents for Romanesque churches in Burgundy and Provence. It seems plausible that they might have done so in Aquitaine as well, without necessarily carrying any specific meaning.

Jonathan B. Riess interprets the 1297 fresco cycle in the Palazzo dei Priori as a reflection of the changing political thinking in the commune of Perugia in the late thirteenth century. Like the Aquitainian façades, the scenes depict easily recognizable biblical subjects as well as more unusual nonbiblical themes. Riess argues quite convincingly that their inclusion in a civic structure and their juxtaposition in the council chamber implies that newly evolved secular ideals guided their selection. Government is viewed as a benign necessity. Models for just military and political leadership capable of condemning tyranny, restoring liberty, and maintaining social order are offered. The redemptive value of serving the state is also emphasized. His explanation for the cycle rests on the major change in the concept, origins, and role of political institutions that resulted from the Aristotelian revival of the thirteenth century and its reinforcement by Aquinas. Additional influences came from imperial and papal propaganda and civic views echoed by chroniclers of the communes.

Although in general the breadth of Riess's approach offers fairly convincing evidence for his assertion that the frescoes were the result of the changing political ideology in Perugia, this study does raise certain questions. Few of the texts mentioned by Riess are cited directly. An inclusion of all the crucial passages in the footnotes or an appendix would have been desirable. Even if the reader agrees that the impressive series of citations explain the frescoes, one is left wondering how, considering the lack of a long-standing tradition, this series of factors coalesced into a pictorial statement in 1297. Riess mentions in an introductory chapter that scholars arrived in the 1290s together with the founding of the university. Because this group acted as historians for the commune, the question of their involvement at the Palazzo dei Priori could be

raised. Another problematic area is the insistence by Riess that the frescoes are the product of a general shift in the political state of affairs rather than any specific historical events. The repeated emphasis upon military leadership in both the biblical and astrological scenes suggests that the series of military triumphs enjoyed by the commune in the period immediately preceding 1297 might have had a bearing upon the choice of subjects.

Despite certain limitations, both studies are valuable contributions because of the way the authors interpret these monuments as articulate statements about the powers, privileges, and aspirations of certain segments of medieval society. Because they deal with visual evidence from a period where a consistent textual tradition is lacking, they form a new type of documentation for scholars working in historical as well as art historical fields.

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MAUREEN FENNELL MAZZAOUI. *The Italian Cotton Industry in the Late Middle Ages, 1100–1600*. New York: Cambridge University Press. 1981. Pp. xiv, 250. \$49.50.

It is generally agreed that the textile industries were the most productive, diversified, ubiquitous, and influential sector of medieval manufacturing, but not all branches have attracted the same attention. Thanks to its size and belligerency, the woolen industry and trade has been thoroughly studied. Silk fabrics hold the next place because of their sophistication and glitter. Linen, which includes expensive products and was exported as far as China, has been studied competently but spottily. Cotton and fustian, aptly called by Maureen Fennell Mazzaoui "the clothing material of the common man," has long been the cinderella of the sector. Two brilliant but short essays by Franco Borlandi (respectively in the *Studi Gino Luzzatto* and the *Hommage à Lucien Febvre*) had whetted our appetite for a book he unfortunately did not complete. Then, in 1978, Wolfgang von Strömer devoted to cotton manufacturing in late medieval "Mitteleuropa" (mainly the German-speaking countries) a massive monograph, which is somewhat impaired by ponderous style and retroactive patriotism, but which lays the foundations for the study of one of the two major areas of medieval production.

In a spirited and readable way, Mazzaoui's new book does the same for the other major area, Italy, with thinner documentation, because Italy had an earlier start and the most prominent centers (Cremona and Pontremoli) do not have the fabulous archives of some other Italian cities. She appropriately starts from the production of the raw material

and examines its trade, its manufacturing, its technological and corporative organization, and its rise and decline. She also makes excursions into other Mediterranean countries that made or traded cotton ware. Cotton served in many ways: as stuffing for pillows and mattresses, in sailcloth and cords for bows, in candlewicks, for tablecloth and napkins, and so forth. Its widest use, however, was in cheaper kinds of clothing. Its exploitation on a larger scale than would have been possible with more refined wares enabled many merchants and textile entrepreneurs to reap good profits, gave employment to many workers of moderate skill in city and country, and (last, not least) filled ships insufficiently steadied by light, precious commodities with something better than ballast. More ubiquitous, perhaps, than the other three sectors, it does not seem, however, to have supported merchants and cities as affluent as the centers of wool, silk, and linen. Yet cotton had been a luxury in antiquity and in its best quality a treat in early medieval Islam.

Mazzaoui is right in ascribing this progress to technological innovations, first on Islamic and Indian plantations, then in Italian workshops, but I do not think she quite explains the reasons why the Italian manufacturers discarded the more sophisticated varieties of fabrics and the more expensive dyestuffs included in the Islamic centers. Wool, not cotton, was the raw material that the climate and taste of Italy and its northern customers favored; "everything else being equal," as economists say, cotton could compete only if its price was lowered as much as possible by standardization and plainness. These, for better or for worse, are the characteristics of industrial development and mass production; therein was the chance for what one might call economic democratization. Indeed, when the depression of the Renaissance (or, as a Marxist would maintain, the "rise of capitalism") increased in Italy the distance between the rich and the poor, Italian entrepreneurs tended to drop cotton for more costly textiles and thus let their supremacy in that sector pass to less-developed Germany. But this disagreement (if it is a disagreement) and some minor slips not worth mentioning in a brief review do not in the least diminish the value of an altogether excellent book.

ROBERT S. LOPEZ
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MODERN EUROPE

STEVEN OZMENT. *The Age of Reform, 1250–1550: An Intellectual and Religious History of Late Medieval and Reformation Europe*. New Haven: Yale University Press. 1980. Pp. xii, 458. \$25.00.

This book is a splendid and masterful survey of late medieval and Reformation religious thought. It is notable for its lucid exposition of intricate intellectual problems, for its strong opinions and theses, for its courage, and for its breadth and depth of learning. Steven Ozment has had the temerity to present to the public the results of a decade of lectures to students at Yale. Drawing on his own specialized research in late medieval thought, defining his own position in a highly self-conscious way against that of many others, especially of the social or economic historians, countering even his mentors, and inviting debate, he has presented in this volume a highly discussable and spirited interpretation largely of the religious thought of the period.

There is precious little on the Renaissance, art, letters, or science, which also belong to the intellectual history of the period 1250–1550, as most secular historians will affirm. Ozment is unnecessarily defensive about defending intellectual ideas and religious history against the “sociologizers,” when in a recent review he labeled them as “reactionaries” (Brady, von Greyerz, Rublack, and others). For although we have once again (the new history from “the bottom up” is not so new as its ingenuous protagonists imagine) an emphasis on history’s “many huts” rather than history’s “many mansions,” most social historians recognize the independent life and, through a trickle-down process, at least, the importance of ideas and words in human social history. The enterprise is justified in its own right and not by faith alone.

The special strength of the book lies in its brilliant treatment of late medieval thought, its engagement with the large questions posed by the old Catholic and new Catholic, the old Protestant, and new Protestant points of view on the quality of fourteenth- and fifteenth-century thought, and the nature of late medieval religious life—questions such as how oppressive for the laity was the fear-motivated piety and the penitential system. Even there attention to Millard Meiss on the Black Death, low life expectancy, and scorbutic tendencies would flesh out the argument. Ozment comes close to Gilson’s position, but with important positive differences in the evaluation of the fourteenth and fifteenth centuries. The treatment of the importance for the Reformation of humanism in contrast to scholasticism is not sufficiently radical. Luther’s bold act came by way of a rejection of “scholastic rubbish” and was made possible by the new humanist approach to the texts of the Scriptures. The book becomes progressively weaker in its treatment of Zwingli, Knox, the English Reformation (virtually nothing at all), and the nascent Catholic reform. It offers some stimulating and highly controversial assessments of Calvinism in relation to the initial questions at issue between Luther and medieval

theology on grace and good works, although Calvinism as a historical movement lies mostly beyond the *terminus ad quem* of this volume.

The concluding chapter on the legacy of the Reformation is a gem: judicious, perceptive, and, to my mind, basically correct. If it only could have been longer! Overall, this is a book that reveals a strong mind, great learning, and a courageous spirit ready to make a stand and not just assume changeable positions, and it is beautifully written, though hastily toward the end. Yale Press provides some comic relief with such howlers as Reginald Pope (p. 402) and Martin Buber in Strasbourg (p. viii).

LEWIS W. SPITZ
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W. ROSS JOHNSTON. *Great Britain, Great Empire: An Evaluation of the British Imperial Experience*. (Scholar’s Library.) New York: University of Queensland Press. 1981. Pp. xvii, 207. \$38.85.

In this small, 200-page volume, W. Ross Johnston attempts to break free of controversial anti-imperialist theories that have been propounded by Marxists and colonial nationalists and to provide a fresh assessment of the British imperial experience from Tudor times to the present. He concludes that, although there were undoubtedly individual acts of destructiveness and mistreatment of colonial peoples, the British empire on the whole was moderate, benign, not particularly exploitationist or economically motivated, administratively inefficient, and indeed rather passive. Except in the white settlement colonies and the West Indies, the British did not sink deep roots; the colonized therefore preserved their cultural autonomy and thus the means for establishing political independence.

The intended audience and purpose of this book are not entirely clear. The volume is included in a “Scholar’s Library” of specialized works, available only from the publisher, and its length is appropriate to an essay. But the level of interpretation is thin, the conclusion summarized in the previous paragraph being the only central theme. Johnston has few ideas of his own, and he either neglects entirely or disposes in a couple of sentences of the more interesting recent interpretations of others: the ecological approach of Alfred Crosby’s *Columbian Exchange*, the world-system perspective of Immanuel Wallerstein, the emphasis on collaborative mechanisms developed by Ronald Robinson, or the dynamics of the frontier as argued by Donald McIntyre (not to mention Sir Keith Hancock). The enormous complexities of Britain’s several centuries of relations with peoples and cultures around the world are here reduced to simplistic, self-evident platitudes. The blandness of the argument is illus-

trated by the following passage: "British imperialism operated under a dual approach—directives from London and initiatives from the periphery—because there were two different views as to the position of Britain's power in the imperial setting: a more secure one reposing in London, a less assured one generating in the colonies" (p. 52).

Johnston has written not so much an interpretive essay as a sort of handbook. The specialist will learn absolutely nothing. A more general reader will find recent surveys by Ronald Hyam, Bernard Porter, and Donald McIntyre much more satisfactory, though they deal only with the later period. For the whole expanse from Tudor times on, C. E. Carrington's *Englishmen Overseas: Exploits of a Nation of Shopkeepers* has still not been superseded. It certainly should be. But Johnston's disappointing book does not fill the bill.

JOHN W. CELL
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E. I. KOURI. *England and the Attempts to Form a Protestant Alliance in the Late 1560s: A Case Study in European Diplomacy*. (Suomalaisen Tiedeakatemia Toimituksia, series B, number 210.) Helsinki: Suomalainen Tiedeakatemia. 1981. Pp. 219.

This generation of historians of early modern England has made a spectacular harvest of social history, but while the social historians have turned inward for sources and outward for tools, some fields have almost disappeared for want of cultivation. When diplomatic history occupied a better place in the sun, English historians usually worked France, Spain, and the Low Countries, leaving the Baltic to commerce, Germany to religion, and Italy to literature and the arts.

E. I. Kouri's book is a reminder that to Elizabeth and her ministers, the secondary importance of the German and Baltic states was importance nonetheless. He concentrates a breadth of archival research into a detailed account of Anglo-German diplomacy in 1568–69. Although some consideration is given to Scotland, the Baltic states, France, and Spain, attention remains on the relations between England and the German Protestant states and relations among the German Protestant states.

The military successes of Alva in the Low Countries and the military defeats of the Huguenots in France left England and the Calvinist Palatinate diplomatically isolated and militarily insecure. Palatinate diplomacy, seconded by Dutch and Huguenot representations, looked to England for aid, and England looked to the German Protestants as a way to overcome isolation. Kouri clarifies the complicated negotiations among the German states that produced the Erfurt meeting where Saxony's influence

upon the other Lutheran princes rejected interest in anything other than the religiously balanced empire previously secured at Augsburg. It is true, as Kouri observes, that Germany thereby enjoyed fifty years of peace, but it is also arguable that Saxony's neutralism as well as Palatinate adventurism contributed to the growth of the Counter Reformation in the empire, so that when peace ended in war, it was a war that seemed never to end. For England the failure to find an alliance in Germany was a step toward recognizing that Spanish power in the Low Countries required a better understanding with France now that the ancient enemy did not threaten from Scotland. The conference at Erfurt began a half century of peace for Germany and a generation of war for England.

Kouri's impressive research shames insularity and enlarges the understanding of sixteenth-century diplomacy. The narration of a many-sided diplomacy inevitably is repetitious, and that difficulty probably will remain when Kouri undertakes his promised larger study of Elizabethan diplomacy. For that larger study he might consider placing in an introduction the important but scattered background treatments of topics such as the factions in Elizabeth's council and the religious divisions among the Lutheran princes. Such topics then would inform rather than ambush the reader. The chart of currency comparisons is helpful, but maps and some German genealogies also would have made this valuable study more useful.

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ROBERT K. FAULKNER. *Richard Hooker and the Politics of a Christian England*. Berkeley and Los Angeles: University of California Press. 1981. Pp. vi, 190. \$22.50.

Richard Hooker is a thinker much more often referred to than read. This condition may improve with the Folger edition of his works, and Robert K. Faulkner's book is another example of reviving interest. In many ways a conventional study, it stakes its reputation on trying to show that Hooker was criticizing the Elizabethan order as well as defending it and that he saw Erastianism and laicism as enemies almost as dangerous as Puritanism. Hooker was vitally concerned with lay (including royal) encroachment on ecclesiastical power and wealth, and Faulkner finds—especially in book 7 and the mangled book 8 of the *Laws*—a very clerical Hooker, whose circumspect but substantial program for ecclesiastical reconstruction points to Laud's policies, almost to theocracy.

The evidence is there, but its weight is questionable when compared with the preponderance of anti-Puritan argument. Other clerics dreamed of clerical glory and larded encomia to their church with bitter asides about the hard-hearted laity. Is Hooker so unusual in this respect? And is this version of Hooker so different from previous ones? Sometimes the attempt to claim originality is strained, and the views attacked are not so far from those adopted here.

To a historian, this book is often vaguely annoying. There is a profitless preoccupation with figures of largely historiographical interest, from Hume to A. F. Pollard, and an over-careful and self-conscious reference to a few quite reliable scholars, too often on matters that will be known to anyone likely to read this book, which looks like the work of a political scientist managing rather well on unfamiliar historical ground. The lengthy section on ethics is impressively done but not very germane to the whole, and the last part is particularly unsatisfactory; the section on the royal supremacy should have come earlier, and the ending is very abrupt. Mistakes are few, though within a few pages we are introduced to odd things called "limited cogs" (p. 169), the advance of the House of Commons is overrated and the fourth duke of Norfolk made into a Catholic (p. 172), and Hooker is "tasked" (p. 182) when he is meant to be "taxed." More serious is the lack of a bibliography.

Most of the book is reliable and straightforward, however, if not particularly original. Whether the matter of Hooker's hitherto underestimated criticism of his own establishment justified a whole book is another matter.

JAMES DALY
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GRAHAM PARRY. *The Golden Age Restor'd: The Culture of the Stuart Court, 1603–42*. New York: St. Martin's Press. 1981. Pp. xi, 276. \$27.50.

The ambitions of King James I to restore and maintain peace and the arts in his kingdoms were celebrated in the masque, *The Golden Age Restor'd*, created by Ben Jonson and Inigo Jones and presented at court on Twelfth Night, 1615. From this masque Graham Parry takes the title of his book. Its scope, indicated by its subtitle, *The Culture of the Stuart Court, 1603–42*, is further defined as limited to the main lines of development in literature, architecture, and painting, and specifically excludes analysis of the state of music.

Parry's first chapter, "The Iconography of James I," presents a good example of his approach. It starts by describing and analyzing James's triumphal entry into London in 1604. "Combining architecture with emblem, tableau, drama and music, the

event demonstrates compactly how the arts served the monarchy by projecting a state mythology, and also offers a view of the iconography prevailing at the beginning of James's reign" (p. 1). The chapter ends with an account of "the posthumous celebration of the achievements of his reign" as portrayed by Rubens in the Banqueting House. "The commanding centre of the ceiling shows the Apotheosis of King James. . . . For a King who ruled by divine right, apotheosis was the perfect consummation. . . . Thus the familiar elements of Jacobean iconography are gathered in one last splendid scenario. Unity, Peace, Plenty, Religion, Justice, Wisdom (with the Solomonic allusions), Empire, the Golden Age restored, these are the achievements of the Stuart succession. . . . Seated in his Roman hall beneath his deified father, Charles was surrounded by the rhetoric of absolute power" (pp. 32–37).

Chapter titles further suggest how Parry marshals his evidence; they include "The Jacobean Masque," "The Court of Henry, Prince of Wales," "The Wedding of Princess Elizabeth," "Thomas Howard, Earl of Arundel," and "The Duke of Buckingham as Collector and Patron." Inigo Jones, who set "the visual tone" for the court, and Ben Jonson, who expressed the complementary morality, "Roman, ethical and severe," each receive a chapter. The first half of chapter 9, "The Court of Charles I," discusses the Caroline masques, all of which were presented in the period of rule without Parliament and which "act as a vindication of royal autocracy, not by an explicit defence of Charles's political actions, but by an assertion of powers so sublime that their exercise is inevitable, irresistible and benign" (p. 184). It is followed by "The Religious Arts under James and Charles."

In his conclusion Parry asserts that culturally James's court stood higher than Charles's and he suggests that differences between the culture of the reigns may derive from the contrasting temperaments of the kings and their education. James was "a static figure invested with a distinctive iconography," while Charles tended to make a dramatic presentation of his kingship either in a masque or in a painting. Parry ends by quoting from Marvell's "Horatian Ode" a passage that includes the particularly appropriate lines "That thence the *Royal Actor* born/The tragic Scaffold might adorn" (pp. 267–68).

Twenty-one illustrations and a select bibliography (but no select list of phonograph records) contribute to the usefulness of the book. Readers with a basic knowledge of the history of the period, as well as specialists in aspects of it, will find it unusually attractive and valuable. Syntheses of this quality are rare.

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JOHN K. GRUENFELDER, *Influence in Early Stuart Elections, 1604–1640*. Columbus: Ohio State University Press, 1981. Pp. xv, 282. \$22.50.

Parliament, in a phrase of G. R. Elton more than once cited by John K. Gruenfelder, was a "point of contact between rulers and ruled." Neither the gentry's ambitions for a place in Parliament nor (despite some of the more strident revisionist claims of recent years) the importance attributed to Parliament can be denied. It is precisely the interaction of these two factors that provides the context for this study of the operation of influence and patronage on parliamentary elections under the first two Stuarts. The House of Commons increased in size between 1604 and 1640, but only by twenty places; in such a situation the competition that the gentry faced in their bid for seats was bound to increase, and, if it were the case that the influence of the peerage remained extensive and the intervention of the court grew, the pressure would be even greater. Such pressure led to something for which the elite had little desire, namely contested elections. Given the fact that the electorate was surprisingly large as well as increasingly aware and responsive, it is hardly surprising that the leadership of the country was more interested in pre-election consultations, negotiation, and agreement than it was in an unsettling and factious contest.

How did patronage and influence, which could make the path to Westminster a less bumpy one, actually work under the first two Stuarts? That is the question Gruenfelder proposes to answer in this exhaustive study of the elections that took place in England between 1604 and 1640. He argues that patronage and influence dominated parliamentary elections in the period. But the rivalry for electoral influence among the elite was fierce, for the gentry were confronted with other patrons in the peerage and, in contrast to the Elizabethan past, in the court itself. As early as 1614 the success of such influence occasioned the expression of fear for Parliament's survival as a "free" institution. Such fears, as regards the peerage, were especially to the point, Gruenfelder argues, in the Parliaments of 1624–28 "when the peerage was consistently involved in more than 45 percent of all elections for parliament" (p. 213). In 1628 the picture changed; intervention remained at its customarily high level but the number of places won or probably won declined about 20 percent, and the downward trend continued in the elections of 1640. The case of court electioneering followed a not dissimilar pattern. Gruenfelder argues that court electioneering, especially as practiced from 1614 through 1624 and again in 1640, was the notable innovation of early Stuart elections. From 1614 through 1624 the court was involved in 17 percent of all elections; in 1640 the figure approached 18 percent. But the success

rate was considerably higher between 1614 and 1624 than it was in the two elections of 1640 when an average of intervention in 46 elections produced only 26 places.

The sharp falling off of the capacity of crown and peerage to exert effective influence and patronage in 1640 was doubtless, as Gruenfelder acknowledges, a product of broad political concerns, but it was also, he suggests, an outcome of the county community's perception that its local autonomy was threatened by such patronage. On this particular point the success or failure of outsiders in elections provides a key test, for victory for an outsider meant in most cases a victory for the competitors of the local elite: the peerage and the court. On average 122 outsiders were chosen for each Parliament from 1604 through 1628; in 1640 the figure fell by about one-third despite the unprecedented electoral activity of that year.

From his analysis of these results, Gruenfelder concludes that the elections of 1640 marked a new world in terms of patronage and influence. The influence of the great peers had declined, as had that of the court; the Duchy of Cornwall was humiliated, the Duchy of Lancaster returned fewer candidates than in any other early seventeenth-century election, and the lord wardens experienced a similar fate. "The influence that counted in 1640 was the influence of the gentry, of the county residents who, because of their close ties to their communities, were regarded by an active and interested electorate as the men who could put things right in a troubled England" (pp. 217–18).

This is an extensively researched and, on many counts, useful study. But it is not without its problems. They mainly stem from the difficulty in distinguishing between friendly letters of support and genuinely decisive intervention. It may well be that, in most cases, the surviving evidence does not allow such a critical distinction to be drawn, but that circumstance would argue for caution rather than for a decisive statement. The case of Richmond in 1640 is illustrative of the problem. In appendix 6, both members returned to the Long Parliament are identified as elected as the result of the intervention of a royal agency. Both (Sir Thomas Danby and Sir William Pennyman) were supporters of Strafford. Both were also county residents of considerable local importance who might well have been elected without any exterior help. That they received such endorsement is perfectly true, but to argue that they were elected because of it seems less certain. Danby admittedly had not served in Parliament before, but he had been a JP in the North Riding and a deputy lieutenant and his father-in-law had represented the town in the past. Pennyman had been sheriff, was a substantial landholder, and had also been a deputy lieutenant. The situation is all the more uncertain because of the way in which the borough had so

successfully resisted the pressure of the Council of the North in the past. It must be said that Gruenfelder does in the text recognize this problem; "possibly," "probably," and "presumably" are frequently used terms with respect to the use of influence. The frequency with which they have to be used may suggest the danger of drawing too definitive conclusions from the mass of evidence presented here; out of nine cases of electoral involvement attributed to the Earl of Derby, six are listed as probable, three as possible, and none as definite. There are, of course, numerous cases of clear intervention, successful and unsuccessful, and the broad outlines of the system of patronage and influence are plainly delineated. But as the case of the Earl of Derby suggests, some of the finer workings of the system remain in a somewhat less definitive status.

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JOHN CHILDS. *The Army, James II, and the Glorious Revolution*. New York: St. Martin's Press. 1980. Pp. xix, 226. \$25.00.

In just under four years James II quadrupled the English army, brought in 5,700 soldiers from the Irish and Scottish armies, and recalled six regiments that had been in Dutch service. The total number with which he opposed William of Orange was 29,000–30,000. The present work describes the growth of this force, characterizes the officers, and shows how far the army conspiracy against James was the result of his own actions. To a large extent John Childs's findings rest on biographical data relating to the officers. The separate publication of these materials would provide correction for many articles in the *Dictionary of National Biography* and would valuably supplement most of the published histories of the regiments raised before 1689.

Childs found that of 1,869 officers in the English army in 1688, 209 were Roman Catholic. Catholics constituted, that is, 11 percent of the officer force in 1688 as compared with 10 percent in 1685. The figures refute any idea that James II was intent on sweeping out all the Protestants, though the author's analysis of the individuals involved does reveal that the officers in 1688 were far less representative of the social and political establishment than their predecessors had been under Charles II.

In the case of Ireland the purge of the Protestants was undisguised, and more than 7000 officers and men were dismissed, to be replaced by Romanists. Although about 50 of the 406 new Irish officers were experienced professionals, the vast majority lacked military training. Discipline was bad, and

there were many complaints from civilians about the excesses of the Irish soldiery.

Childs maintains that James intended to use the army as a political force, and he shows, for example, that its members were strongly relied on in local affairs. Civil-military relations were colored by the king's desire for military autonomy. In the interest of discipline he introduced martial law in time of peace. Childs believes it was his intent to go even farther, and to bring all litigation involving the army before courts martial, even where civilians were party to disputes. The "definitive" evidence cited is a remark of George Clarke, judge advocate, recorded in his autobiography in 1720, but the royal warrant setting up the weekly court martial does not support this interpretation; it directs the court to hear civilian petitions or complaints against the military and to report thereon to the king, who will give further directions as justice requires.

Macaulay regarded the desertion of James by army officers as part of the national movement to preserve religion and liberty, but Childs denies this. "In 1688 only a tiny number of English officers revolted against James," he writes (p. 85), and he goes on to assert that the mutinous officers were motivated by adventurism and fears of further change in the officer corps rather than by principle. Nevertheless, the effect of their desertion was unmistakable in that it damaged the morale of the king and spirit of the army.

An original approach coupled with exact and detailed research make this a solid contribution to the history of the Revolution of 1688–89.

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ANN KUSSMAUL. *Servants in Husbandry in Early Modern England*. (Interdisciplinary Perspectives on Modern History.) New York: Cambridge University Press. 1981. Pp. xii, 233. \$29.95.

This study by Ann Kussmaul of an occupational group that long constituted a highly significant component of English society spans the centuries from the sixteenth to the mid-nineteenth. The first systematic investigation of the subject in seventy-five years, it is able to employ materials and methods unavailable earlier. Indeed, it reflects throughout the profound influence of the Cambridge Group, and it forms part of a series denominated "interdisciplinary perspectives." It therefore self-consciously represents a distinctive, modish historiographical genre.

The study opens with an explanation of the essential problems that confront the investigator of servants in husbandry: the dearth of evidence, the difficulty of defining the occupational group, and the manifold ambiguities of contemporary terminol-

ogy regarding its position in the social order. This propaedeutic sketch is followed by a series of chapters on the group in early modern times. The first of these treats its magnitude and incidence according to geographical area, size of farms, sex, and age; moreover, this account likewise explicates the various conditions that fostered its existence as well as the different functions that it fulfilled. The next chapter examines both the character of service in husbandry as an institution—especially its legal aspects, contractual bonds, and economic rewards—and the nature of the tasks performed by those engaged therein. This is followed by a twofold chapter detailing how servants in husbandry were hired and describing their temporal, spatial, and social mobility: how often they changed employment, how far they moved when they did so, and how they acquired or lost status. The penultimate chapter then charts the changing incidence of service in husbandry between 1540 and 1790. Finally, an epilogic essay explains its extinction in the north and east between 1815 and 1851.

The fundamentum of this admirably intensive and comprehensive study is constituted of a wide variety of quantified data, often derived in the most ingenious manner, invariably employed with the greatest circumspection, and at times undervalued as evidence in a zealous quest for maximum reliability. These inevitably scattered and fragmentary sets of data are morticed to judiciously selected descriptive material with striking skill and dexterity, so that the whole structure possesses an impressive unity, coherence, and substantiality; moreover, it inspires confidence. These characteristics make the study at once a valuable work of English agrarian history and an exemplary specimen of the potentialities of quantification, when carefully applied to the records of the past.

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P. B. MUNSCHÉ. *Gentlemen and Poachers: The English Game Laws, 1671–1831*. New York: Cambridge University Press. 1981. Pp. ix, 255. \$37.50.

Hunting laws have been a fact of English life since the Norman kings safeguarded their hunting monopoly with the notorious forest laws. That monopoly did not outlast the Middle Ages, but the transfer of sporting privilege from the crown to the landed gentry was not completed until the Game Act of 1671. That act, which in Blackstone's famous phrase "raised a little Nimrod in every manor," is the starting point for this study. It concludes with the Game Reform Act of 1831, which terminated an era during which the taking of game in England was the exclusive privilege of the landed gentry.

First and foremost, then, P. B. Munsche's book is a study of the English squirearchy, of their values and of the ways in which they sought to impose them on eighteenth-century society. But it is very much more. It is a model of historical writing: graceful, economical, and objective. It is also a genuine, but never self-conscious, revision. In seven compact chapters and an appendix of the laws passed between 1660 and 1831, Munsche replaces the confusion and literary-based generalizations of the Hammonds and Webbs with a clear and authoritative account of what is now revealed as a complex and pervasive social, economic, and constitutional problem. Most Englishmen believed that hunting was a natural, God-given right to be enjoyed by all. Game laws therefore conflicted directly with fundamental popular belief. The gentry, on the other hand, saw them as "designed to preserve a stable society, one which was rural-based, hierarchical and paternalistic" (p. 7). It was a question of preserving social order, and not until the gentry were finally convinced that the game laws were endangering that social order did they agree to their reform.

Munsche reveals the many anomalies and tensions generated by that basic conflict. The replacement of nets and hawks with guns and beaters from the mid-eighteenth century onward inexorably escalated hostilities. Wholesale slaughter necessitated the systematic breeding and preservation of game. That in turn alienated the farmers and small freeholders who saw their crops trampled by hunters or eaten by hares, pheasants, and partridges that they themselves were forbidden to hunt. Attempts to stifle the market in game, culminating in 1755 in a total ban on selling game, made customers wholly dependent on poachers for supply. A lucrative black market was quickly organized, as were poaching gangs and a corps of gamekeepers to combat them. The result was the savage "game wars" of the late eighteenth and nineteenth centuries.

Popular ballads such as "The Lincolnshire Poacher" and the writings of Richard Jefferies have cast a romantic glow over the brutal nocturnal battles between poachers and keepers and the man traps and spring guns with which coverts were sown almost into living memory. Under Munsche's dispassionate eye that dark side of English rural life re-emerges in vivid detail. On one level it is a predictably grim tale of inhumanity in the interest of maintaining a class monopoly. But in a chapter that merits detailed attention Munsche also carefully exposes the discretion with which the game laws themselves were enforced. The result is a balanced, compelling account that deserves to become the classic treatment of this fascinating topic.

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COLIN A. PALMER. *Human Cargoes: The British Slave Trade to Spanish America, 1700–1739*. (Blacks in the New World.) Urbana: University of Illinois Press. 1981. Pp. xv, 183. \$19.50.

Colin A. Palmer has written a first-rate book on a segment of the British slave trade to Spanish America. Under a slightly misleading title he has in actuality explored the slave trade under the *asiento* held by the South Sea Company from 1713 to 1739.

The *asiento*, giving England the sole right to deliver slaves in Spanish America, was secured in the treaty of Utrecht in 1713. It has traditionally been interpreted as a diplomatic prize valued for the opportunity to vend nonhuman cargoes in Spanish America. In this interpretation few slaves were delivered, and the slave trade was unprofitable. Though there have been many diplomatic and commercial studies of the British *asiento*, Palmer's is the first study of the slave trade under it. He qualifies the accepted view by investigating the South Sea Company papers in the British Library, manuscripts in the Public Record Office, and, in particular, Spanish sources in the Archivo General de Indias in Seville. He also drew on the Shelburne Manuscripts in the William L. Clements Library at the University of Michigan.

In describing the trade in Africa Palmer observes that England maintained good relations with blacks; that the slave trade, instead of being a principal cause of intersocietal war, "supplemented but did not replace the essentially local reasons for warfare in West Africa" (p. 24); and that remote Angola, under Portuguese influence, not British settlements in West Africa, was the leading slaving region for South Sea Company ships. In telling of the notorious Middle Passage Palmer follows recent scholarship in stressing heavy mortality before and after embarkation, in discounting overcrowding as a cause of death, and in considering the problems of disease environments and tropical medicine.

A chapter on the structure of the *asiento* trade in the Americas describes the company agencies in Barbados and Jamaica and the factories in Spanish America through which slaves were sold. As early as 1716 the company, though a monopoly that preferred to sell in large parcels, resorted to a licensing system under which individuals could sell slaves in designated areas. Never able to fulfill its contractual quotas, the company in its own interest sought to suppress illicit slave trading but was unsuccessful against the odds of foreign (French, Portuguese, and Dutch) traders, private British traders, and its own employees. The trade was subject to unceasing strains with the Spanish crown, Spanish American officials, and the Jamaica Assembly, as well as with foreign and British interlopers.

Spanish buyers wanted young, healthy, and

strong slaves; the bulk of the slaves sold by the company ranged from 10 to 25 years of age. Palmer, drawing on port records in the Seville archives rather than the *asientos*, breaks new ground in detailing the numbers of slaves delivered; no previous writer has given a systematic account of the volume of the trade. With the ports of Panama and Porto Bello (combined), Buenos Aires, Cartagena, and Havana leading, Spanish America received from the South Sea Company 74,760 slaves between 1714 and 39. Palmer suggests that this makes Philip Curtin's benchmark estimate of Spanish American slave imports, comprehending both the illegal and legal trade, "too low for the *asiento* years" (p. 111). Taking up the question of profitability, he concludes that "the company's venture into the slave trade was far from unprofitable," in fact, "better than good" (p. 155).

In summing up, Palmer, observing that the South Sea Company was an anachronism in an age turning to private trading, maintains not only that the company delivered a substantial number of slaves to Spanish America, but also that "the *asiento* engendered a major expansion in England's share of the international slave trade."

The general excellence of this monograph would have been enhanced had the author provided a discussion of historiography, had he not asserted that the Dutch never held the *asiento*, and had he amplified such themes as the activities of English and Jamaican contractors, the African transaction, and sales in Spanish America. In spite of these shortcomings he has made a significant contribution to a subject frequently referred to but never before methodically examined by a historian.

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CRAIG CALHOUN. *The Question of Class Struggle: Social Foundations of Popular Radicalism during the Industrial Revolution*. Chicago: University of Chicago Press. 1982. Pp. xiv, 321. \$25.00.

The hounds have long since picked up the trail of the wily E. P. Thompson, and now Craig Calhoun joins the field of hunters riding hard in pursuit. Some of the best parts of this young sociologist's book consist of his perceptive analyses and criticisms of Thompson's work, above all *The Making of the English Working Class* (1963). Calhoun chides Thompson for his "somewhat chaotic" organization, his disdain for abstract argument, and his persistent problems with logic, coherence, and consistency. The sly old fox is not taken yet, of course. And Thompson will probably prolong the chase by offering new descriptions *qua* definitions of class con-

sciousness, struggle, and all the rest. Are not backtracking and deception part of the polemical sport? Yet even those out of sympathy with the fuss made over the pursuit of the inedible should have a look at Calhoun's first major outing, for he strives to be the compleat huntsman by composing clever lines against Neil Smelser, John Foster, Karl Marx, and many others, while simultaneously galloping after his main quarry.

Calhoun is somewhat less successful, though, in his quest for "the social foundations of popular radicalism" during the English Industrial Revolution. His complex argument incorporates the increasingly popular notion that the years around 1820 comprised a watershed in English social history. Calhoun views radicalism in the period before that time as being vehement in its opposition to changes that directly or indirectly threatened traditional occupations, beliefs, and social relationships. As such, it was backward-looking and was promoted by men and women whom Calhoun calls "reactionary radicals" or "populists." These people were typically to be found in communities of urban artisans or in village communities dominated by domestic outworkers. After about 1820, Calhoun sees "new workers" coming to the fore. Employed in increasingly large work units (if, indeed, not in giant factories), these people created radical movements based less on communities than on associations, a category that included labor unions and political reform clubs. The new workers were forward-looking. They tended to accept industrialization as a fact of life and proceeded to seek limited changes within the new socioeconomic order. Yesteryear's radicalism, in short, was evolving toward Victorian reformism.

Although this explanatory framework seems plausible, nagging questions remain about the numerous exceptions that it fails to accommodate and that Calhoun rarely discusses. In calling the radical handloom weavers of the late eighteenth and early nineteenth centuries "backward-looking," for example, Calhoun does not take into account the diversity of their activities, which ranged from the formation of trade unions to agitations for narrow sectional relief to demands for broad-based political reform. It is difficult to see how all of this can simply be labeled "backward-looking." Calhoun likewise fails to come to grips with the Owenite movement of the 1830s and 1840s, which may have rivaled Chartism in terms of size and influence and which, arguably, looked both forward and backward.

The author's neglect of many of the subtleties of his topic may have resulted from the rather limited range of primary sources he employs. Moreover, it is perhaps because Calhoun has not delved deeply enough into historical (as opposed to sociological) works that he falls into error on more than one

occasion. This pattern begins, unfortunately, on the first page of his text (p. 3), where he mistakenly claims that the Blanketeers' march occurred after the Pentridge Rising. He goes on to state—incorrectly—that William Cobbett brought Tom Paine's ashes back to England in 1819 (p. 94) and that Tyburn tickets were "bogus records of felonious criminal activity" (p. 171). Such lapses pale by comparison, however, with the intricate interplay of mistakes and misconceptions found in chapter 7 ("Community in the Southeast Lancashire Textile Region"), which amounts to a veritable fugue of misinformation.

Still, this book should be read for its enlightening critiques of others' works and for at least three of the important implications of its argument: (1) that English radicalism did not become stronger or more intense as industrialization advanced; (2) that the dissociating tendencies of industrialization (alienation, anomie, and the like) were not major foundations of radicalism but quite the contrary; and (3) that the disparate movements and goals of working men and women of the period make it well-nigh impossible to speak of a single, unified "working class" at any point in the English Industrial Revolution.

ROBERT GLEN

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GEORGE SPATER. *William Cobbett: The Poor Man's Friend*. In two volumes. New York: Cambridge University Press. 1982. Pp. xv, 310; viii, 311–653. \$49.50 the set.

William Cobbett's lifetime (1763–1835) witnessed seventy-two years of drastic change in Great Britain. A person who attempts a biography of this figure must be able to deal with enormous shifts in the economic, political, and social structure of the country. Complicating the situation was Cobbett's position as a journalist for most of his adult life; he wrote about virtually everything of importance in England and elsewhere in over twenty million words (about half of the total in the latest edition of the *Encyclopedia Britannica*). In addition, Cobbett had an American as well as an English career. Clearly, anyone who writes such a biography must master a huge number of sources that deal with sport, diet, and rural customs as well as the more traditional politics and economics.

G. D. H. Cole did this in the 1920s, and his *Life* was a remarkable tour de force and one of that great historian's best books. However, in the intervening half a century vast amounts of new material have been uncovered about Cobbett and his England. The prospect of dealing with all of this might daunt

any academic, but not George Spater, who comes to history from a business background.

The chief merit of Spater's study is that it includes a considerable amount of primary source material related to both Cobbett's private and public life. Credit must go to the author for tracking down, in libraries on both sides of the Atlantic, letters and documents that were not available when Cole wrote his book. Because of this, Spater's two volumes replace Cole as the authority on the incidents in Cobbett's personal life. Yet despite the wealth of new material, Spater's interpretation is not much different from Cole's.

Clearly, Spater likes his subject; he writes well and makes few mistakes of fact. I believe that he is better on Cobbett's personality than on Cobbett's ideas and better on Cobbett's American experiences than on the more important English career.

I have no trouble with Spater's diligent research into Cobbett's own writings, but his background analysis creates problems. Although there are many fine secondary sources listed in his bibliography, certain essential ones are omitted and others are overworked. For example, Spater's treatment of the Luddites would have been improved if he had consulted Malcolm Thomis's book on the subject. Furthermore, there is a tendency in Spater's book to rely upon what I can only describe as antiques of British historiography. It is touching to see Harriet Martineau's *The History of the Thirty Years' Peace*, published in 1849, repeatedly cited as an authority. The same is true of William Smart's *Economic Annals of the Nineteenth Century* (1910). Also the controversy concerning the effects of the early Industrial Revolution upon factory workers is one of the raging disputes among British historians, but Spater uses little beyond the opening voice in the argument, Arnold Toynbee's *The Industrial Revolution* (1884). For example, on page 291 he uses Toynbee to support a position of Cobbett. This and other problems may be a reflection of an almost total absence of references to articles in scholarly journals, where much of the secondary knowledge of Cobbett's world is to be found. One cannot understand the period without an immersion in this literature. Thus Spater's touch, which is usually sure when he is dealing with Cobbett the man, sometimes deserts him when he considers the background.

Still, this book was well worth writing. Not everyone will agree with Spater's favorable verdict on Cobbett. Like so many others, he virtually ignores Cobbett's negrophobia and almost buries Cobbett's gross antisemitism in footnotes at the back of the book. But in fairness to Spater, he does make clear that Cobbett was often an impossible person to deal with. Cobbett began to be interpreted in various ways as soon as he died, and this has continued down to the present day. No doubt he will arouse

controversy as long as the issues of his time are relevant to later generations. Spater's book is interesting and, up to a point, useful, but it is not the major work for which Cobbett scholars have been hoping.

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JOHN KENNETH SEVERN. *A Wellesley Affair: Richard Marquess Wellesley and the Conduct of Anglo-Spanish Diplomacy, 1809–1812*. Gainesville: University Presses of Florida, for Florida State University at Tallahassee. 1981. Pp. xii, 291. \$21.50.

Richard Marquess Wellesley was a vain, arrogant, autocratic, selfish man whose political career undoubtedly suffered as a result of these personality traits. His four younger brothers all achieved some success in their chosen fields, the most famous being Arthur, the first Duke of Wellington. In spite of his character flaws, Richard used the family connections to gain appointment as governor-general in India in 1797, a post he held until 1805 when, amid controversy, he was recalled. During the following years Wellesley's friendship with George Canning and his outspoken support of the British commitment to the war in the Iberian peninsula led to his appointment in 1809 as ambassador to Spain. Later in the same year Wellesley was recalled in order to join the cabinet as foreign secretary, which post he held until his resignation in February 1812.

Wellesley's career as a British politician and diplomat has never attracted widespread interest among historians of the period, and what has been written of him has been primarily negative. John Kenneth Severn's book is a re-evaluation of Wellesley's conduct of Anglo-Spanish diplomacy between 1809 and 1812. Basing his study on meticulous research in the primary sources of Spain and Britain, Severn argues that Wellesley deserves better than he has received at the hands of previous historians. He was one of the first to conceive of the importance of the Peninsular war in the struggle against Napoleonic France, and he should be credited with breaking with a foreign policy originally developed by William Pitt that emphasized diversionary expeditions to the Continent in support of Britain's European allies. Instead, Wellesley sought to concentrate British resources in Portugal and Spain where his brothers—Arthur, commanding the Anglo-Portuguese army and Henry, Richard's replacement as ambassador to Spain—could best use Britain's support. Wellesley steadfastly defended British Iberian policy in Parliament and opposed any cutbacks that might have jeopardized the war effort there. Although he was no longer in office when Britain's

Iberian policy finally paid off, Wellesley should receive recognition for helping to maintain the Spanish-Portuguese front against Napoleon during trying times.

Severn's book is an excellent study of Anglo-Spanish diplomacy, 1809–12, and of the roles the Wellesleys, especially Richard and Henry, played. While the author may be criticized for his rather narrowly conceived study, he develops very nicely Wellesley's primary concern while at the foreign office. The maps are somewhat annoying because place names do not always correspond to the spelling in the text and significant sites are omitted entirely (Talavera, for example), but this does not seriously detract from what is otherwise a very sound and well-written book.

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MARTHA MCMACKIN GARLAND. *Cambridge before Darwin: The Ideal of a Liberal Education, 1800–1860*. New York: Cambridge University Press. 1980. Pp. viii, 196. \$34.50.

This brief monograph (only 135 pages of text) is not so much a history of the Cambridge "ideal of a liberal education" as it is a book about the intellectual lives and educational philosophies of a few select Cambridge men, "conservative reformers," especially Adam Sedgwick and William Whewell, who "saw the need to improve the *status quo*" yet wanted to preserve the best of the past (p. 15).

"Within the structure of the old forms," Martha McMackin Garland argues, these men "changed Cambridge almost beyond recognition" (p. 27). This conclusion is considerably overstated.

According to Garland, the Cambridge curriculum was based on a unitary view of truth that presumed that natural science and natural religion were two sides of one coin, that the study of mathematics was sufficient to develop reasoning capacity, and that classical languages and literature were sufficient to develop the aesthetic sense. The first thing we learn is that among the Cambridge scholars there was not an overwhelming perception that substantive reform was needed at all; the second is that few of the changes in curriculum and university structure documented herein really constitute reform. The mathematics curriculum, Garland admits, was "not so much revolutionized as it was renovated—no major upheaval in subject matter or teaching techniques or examinations had taken place" (p. 50). Next to Euclid in the common core of general education stood the ubiquitous William Paley. Garland argues that the Cambridge reformers rejected Paley, but she does not emphasize sufficiently that it was Paley's work of utilitarian moral and political philos-

ophy that was dropped and not his books on natural theology, which remained required reading for examination.

Perhaps the single most-debated issue during this period was the religious exclusiveness of Cambridge and Oxford. The primary question at Cambridge was the admission and graduation of Dissenters, but, although the question was debated widely in the 1830s and 1840s, it was not until 1871 that all religious restrictions finally were removed from Cambridge regulations, and it is questionable whether this reform had much to do with the opinions of the Cambridge dons. A second religious issue was the scandalous neglect of theology at Cambridge, especially considering the fact that the majority of its graduates took Holy Orders. Not until the 1840s were candidates for ordination systematically examined in theology; not until 1848 were they required to attend the lectures of divinity professors, few of whom themselves were required to lecture! The most significant theological discussions at Cambridge took place in the context of the extracurricular Apostles Club.

Most of the reforms Garland describes did not constitute meaningful change. There was some tinkering with the tripos and trivial adjustments of the examination system, but, as the author admits, "at mid-century the traditional combination of classics and mathematics—even if assailed—still held pride of place in the Cambridge curriculum" (p. 121). Ironically, what was perhaps the most significant curricular reform of all, the establishment in 1848 of an honors tripos in the natural sciences, is touched upon only briefly.

Though Garland makes effective use of materials from various Cambridge University archives, surprisingly little reference is made to the vast secondary literature on nineteenth-century English cultural, scientific, ecclesiastical, and educational history. The chapter on "The Challenge of Darwin" evinces an insufficient grasp of the problem of the relations of religion and science. Peter Allen's *The Cambridge Apostles: The Early Years* (1978) and Sheldon Rothblatt's *Tradition and Change in English Liberal Education* (1976) are mentioned incidentally, while David Newsome's *Godliness and Good Learning* (1961) and Brian Heeney's *A Different Kind of Gentlemen* (1976) are not mentioned at all.

JOHN D. ROOT
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J. W. BURROW. *A Liberal Descent: Victorian Historians and the English Past*. New York: Cambridge University Press. 1981. Pp. x, 308. \$45.00.

Just half a century after the appearance of Herbert Butterfield's classic essay, J. W. Burrow has written a

superb study of four Victorian historians who expounded a Whig interpretation of English history. Three of them—Thomas Babington Macaulay, Edward Augustus Freeman, and William Stubbs—exemplify Whig history *pur sang*. They wrote in jubilant celebration of the triumph of freedom and representative government. James Anthony Froude qualifies for inclusion on the basis of a more extended definition. Burrow sets each historian in his historiographical and ideological context, reconstructed with an impressive richness of texture, and then examines the history that resulted. He succeeds admirably in conveying “the complex historical experience of seeing one society and culture refracted through another, both of them, in this instance, our own and not our own.”

Burrow is interested in the Whig historians' uses of the past rather than their contributions to knowledge of it. He shows how Whig history commemorated consensus values and attitudes that cut across class and party lines. Bishop Stubbs, a Tory High Churchman, wrote an epic account of the development of constitutional government, beginning with the spirit of freedom in the German forests. His *Constitutional History* is Whig “in its allowance of premonition, in the sense it conveys at times, not obtrusively, of ordained preparation and a kind of striving at once unconscious and purposive.” But Stubbs did not merely impose a Burkean form on the facts. He displayed a scrupulous respect for the integrity of the sources and for the historicity of the social and constitutional arrangements that he was describing. What made his *History* one of the great books of the nineteenth century—Burrow sees the *Origin of Species* as the closest parallel—was the combination of analytical and literary coherence with a formidable command of complex detail.

Having shown how profoundly the work of the great Whig historians was shaped by their society and culture, Burrow assures the reader that it is not his purpose to endorse an extreme version of historical relativism. The disclaimer is welcome, since the book could easily be read in support of the sort of scepticism expressed by Carl Becker in his presidential address to the American Historical Association in 1931. In fact, Burrow's work can itself be seen as an example of the capacity of the professional historian to transcend, to a degree, the limitations imposed by his culture by drawing on the intellectual resources that it provides.

TRYGVE R. THOLFSEN
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W. D. RUBINSTEIN. *Men of Property: The Very Wealthy in Britain since the Industrial Revolution*. New Brunswick: Rutgers University Press. 1981. Pp. 261. \$22.00.

W. D. Rubinstein's *Men of Property* is a piece of original research, painstakingly constructed and, considering the dryness of much of the data presented, lucidly and at times amusingly written. This work, carried out originally in the early 1970s in the form of a doctoral dissertation, was widened and deepened during a stay in England, and Rubinstein handles his sources deftly. The book is the first comprehensive study of major wealth holders in Britain, and it will be of interest to historians and students of elites alike. The author has studied the social and economic data relating to 1,439 millionaires and half-millionaires (those worth between five hundred thousand and one million pounds) who died between 1809 and 1939. The ferreting out of the data must have been a most laborious piece of prosopographical research, and it is a tribute to the assiduity of Rubinstein's combing of the records that he has very few “unknowns” to report. It is, at the same time, a tribute to his critical historical faculties that mere quantitative data do not overawe him. Having established the significance of landed wealth throughout the nineteenth century on the basis of probate alone, he is nevertheless at pains to point out that the figure would be larger by far if the real value of landed property, given in Bateman's *Great Landowners*, had been assessed on the basis of rent rolls rather than estates passing at death.

Rubinstein is not concerned with the *distribution* of wealth, nor does he, rightly, seek to identify wealth in nineteenth- and twentieth-century Britain with economic or political power. He is, however, concerned with the economic and social role of wealth-holding insofar as this can be identified by such categories as the industry in which the wealth was produced and in the religious—and hence motivational—background of the very rich and the light which both throw on the performance of entrepreneurship in Britain. For Rubinstein the two most important findings of the book are the overriding importance of commercial and financial wealth among the millionaires and half-millionaires and the negation, on the basis of the data collected, of the Weber thesis concerning capitalist progenitors.

The strength of the commercial-financial sector—it provided, in the second half of the nineteenth century and in this century, almost 50 percent of all wealth-holders—is, indeed, astonishing even if it is a little far-fetched to regard this as a sign of the survival of mercantile capitalism and, hence, a negation of the accuracy of Marx's idea of the predominance of industrialism for the Victorian economy. An equally striking finding that emerges from this study is the relative absence of Nonconformism among the possessors of great wealth, disproving, so it would seem, the thesis about the crucial significance of the Protestant ethic in the spread of industrial capitalism. Rubinstein's analysis is far-

reaching enough, tracing religious background as far as the grandfather generation wherever possible, but we are dealing with a minuscule sample of the whole of the entrepreneurial class. The proof of the Weber thesis would not seem to lie in the size of individual fortunes in themselves, possibly depending more on the facilities for rapid expansion available in particular industries at particular periods than on the motivation for entrepreneurial conduct in society as a whole.

It is to be hoped that the author will indeed examine the industrial and commercial elite further, and that he will add to the usefulness of his next book by providing a bibliography.

W. L. GUTTSMAN

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J. C. FURNAS. *Fanny Kemble: Leading Lady of the Nineteenth-Century Stage*. New York: Dial Press. 1982. Pp. xiv, 494. \$19.95.

This new biography of Fanny Kemble chronicles and comments upon the life and writings of a bright and compassionate woman of the nineteenth century. During the long span of her life (1809–93), Kemble was known in both England and America as an actress, a skilled equestrienne, wife, mother, divorcée, abolitionist, Shakespearean reader, and, as J. C. Furnas calls her, a “protofeminist.”

Furnas has, for the most part, carefully trod the line between scholar and storyteller in this particular view of Kemble's life. He provides both contemporary and present-day insights into Fanny's life and writings, reaching deep into the pool of resources only skimmed by previous biographers. Furnas incorporates impressions of Fanny with those of her friends and acquaintances to present a clear view of nineteenth-century life, in city and country, on both sides of the Atlantic. He has also provided good treatment of the inspirations for, and reactions to, Fanny's most controversial publications: her journals recording her impressions of life in America, and her position papers on the institutions of slavery and marriage.

A furor followed her honest, but rather negative account of her first few years in America—a “dreadful” country that both attracted and repulsed her with its “barbarisms,” wild natural phenomena, spitting men, and “vapid” women. Only in Boston, where the Beacon Hill Brahmins seemed to be as interesting and intellectual as she, did Fanny feel truly comfortable in America.

It is this complex relationship of this Englishwoman to America and Americans that is the substance of this book and not, as the subtitle would imply, concern with her work as an actress. Although Fanny was introduced to this country as an actress,

she actually spent a total of only a few months of her long life on the stage. She followed her family's established profession purely out of necessity, not out of love for the craft, and if a student of the “Kemble school,” she may be considered somewhat of a truant. Fanny gladly relinquished her life as an actress to marry Pierce Butler, a young, attractive land- and slaveowning man from Philadelphia. This disastrous union provided a good deal of grist for her journalistic mill, for she found that she abhorred the slavery that she daily encountered on her husband's Southern plantation, and she also discovered that her then-liberal notion of marriage as a partnership could be destroyed by a man who proved to be insensitive, narrow-minded, and much less than faithful.

Fanny defended her quest for a divorce and fiercely attacked slavery in influential published works. Her plantation journal is thought to have aided in turning England against the Confederate cause in 1863, and her “protofeminist” writings established her as an important foremother of the women's movement that was gathering strength during her lifetime.

My only quarrels with this lively and quite readable account of Fanny Kemble's life, aside from the aforementioned misleading subtitle, are the occasional ramblings and breaks in style throughout the book and the author's decided penchant for the word “pungent.” Certainly in every other way the turbulent life of this fascinating woman has been given new life by an equally articulate biographer.

NOREEN BARNES LENZ

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R. F. FOSTER. *Lord Randolph Churchill: A Political Life*. New York: Clarendon Press of Oxford University Press. 1981. Pp. vii, 431. \$29.95.

The subtitle accurately describes the character of R. F. Foster's carefully constructed and closely drawn life of Lord Randolph Churchill. The introduction, epilogue, and all eleven chapters carry the word “politics” in their titles. Each chapter is introduced by an apt quotation from a political novel. And Churchill is integrated into the politics of his day, where he becomes not the “misunderstood Prometheus” of his son, or the “thwarted Lucifer” of hostile opinion, but “a more representative figure than he seems.”

Foster does not identify general or constant themes in Churchill's life: “He makes—and un-makes—sense from day to day.” Accordingly, Churchill is not related to the ideas of his day: “his politics remained instinctual, and his interests were visceral.” Such ideas as Churchill did possess were

grounded in pure Toryism; he was not, *pace* Winston Churchill, a preacher of "progressivism" before his time. In his conclusion Foster quotes approvingly an Irish lawyer who worked with Lord Randolph: "His political action was guided purely by party consideration but in this respect he did not differ from his contemporaries, and while he was more capable, he was not less honest than they" (p. 403).

Foster builds up his portrait of Churchill, an entirely convincing one, by the skillful application of the method of Maurice Cowling, now put to the service of biography. There is an almost day-to-day examination of Lord Randolph during the high point of his career, 1884–87. (The early period and the years out of power are treated in more compressed form because they possess "a recognizable pattern overall.") Evidence is gathered from the Churchill archives, but as much devotion is given to the papers of other politicians (28 major collections) and to political journalism, especially Churchill's unedited newspaper speeches. By sketching out the daily dimensions of his subject, Foster is able to resurrect Churchill's political ghost, which, he says, "only fitfully inhabits the immense, whitewashed shrine" erected by his son Winston.

Lord Randolph Churchill contains a number of shrewd observations, supported by fascinating new information. Foster contends, for example, that the most important thing about Churchill's background was that it was ducal, and the second most important thing was that it was impoverished. Because of the latter, Lord Randolph was often in the company of Jewish financiers; the most prominent of these was Nathan Rothschild, to whom Churchill "turned for everything" by 1888, to whom he entrusted Cabinet secrets, the interests of whose firm he pressed while in office, and who was his closest adviser as chancellor of the exchequer. When Churchill died he owed the Rothschild bank £66,000. (The financiers do not appear in the son's study; Rothschild is not even mentioned in the index.)

Winston Churchill's biography of his father is a great work of art, but it was an act of filial piety that conformed closely to what his "emotions wanted to be true." In Foster's biography Randolph Churchill appears more nearly as the kind of man he was.

J. A. THOMPSON
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JEFFREY D. WALLIN. *By Ships Alone: Churchill and the Dardanelles*. (Studies in Statesmanship of the Winston S. Churchill Association.) Durham: Carolina Academic Press. 1981. Pp. xvi, 216.

For decades after the event, Winston Churchill was plagued by the charge that he was responsible for the disastrous Dardanelles campaign of 1915. The purpose of Jeffrey D. Wallin's book is to present an

account of the Allied attempt to force a passage through the straits by means of the navy alone. Wallin also argues that Churchill was right at every stage of this enterprise and that the naval attack might have succeeded if others in authority had been as resolute as he was. The book is well written. The author possesses excellent powers of exposition. His style is clear and lucid. His narrative is based almost entirely upon previously published material.

Nevertheless, two basic flaws in Wallin's approach mar the quality of his work. He adopts a curious rule: "Opinions unknown (and unknowable) by the statesman cannot provide a basis for judging either his thoughts or his actions" (p. 6). Good political biography or political history cannot be written in conformity with such a maxim. Secondly, Wallin argues throughout that Churchill was always correct in his judgments and planning and that anyone who disagreed with him was unreasonable, shortsighted, ignorant, or deficient in courage and tenacity. All must deplore such carping works as Rhodes James's *Churchill, A Study in Failure*, but hagiography of the kind to be found in *By Ships Alone* will not redress the imbalances caused by bias or by errors of interpretation. Wallin should have paid greater attention to Churchill's own analysis of Asquith's character if he wished to present a valid and believable picture of his hero in action. Of Asquith Churchill wrote: "In affairs he had that ruthless side without which great matters cannot be handled. . . . Not 'all done by kindness'! Not all by rosewater! These were the convulsive struggles of a man of action and of ambition at death-grips with events. . . ."

ALFRED GOLLIN
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JAMES LEES-MILNE. *Harold Nicolson: A Biography*. Volume 1, 1886–1929. Hamden, Conn.: Archon. 1982. Pp. xii, 429. \$25.00.

Harold Nicolson (1886–1968) had a long, eventful career as a diplomat, author, lecturer, politician, and political commentator. His *Diaries and Letters*, edited by his younger son Nigel (3 vols., 1966–68), are often quoted by historians, but they pertain only to the period 1930–62. James Lees-Milne, a literary friend of members of the Nicolson family, was encouraged by them to write a full-length biography, and they made available Nicolson's unpublished diaries, correspondence, and other private papers. This first volume, based largely on these primary sources, provides much interesting new information about Harold Nicolson's earlier life.

Nicolson was the third son of the diplomat Sir Arthur Nicolson, first Baron Carnock, who from

1911 to 1916 was permanent undersecretary of state at the Foreign Office and whose biography Harold eventually published (*Portrait of a Diplomatist*, 1930). Educated at Balliol College, Oxford, young Nicolson passed the Foreign Office examination in 1909 and was appointed to the diplomatic service. While serving as a junior clerk at Whitehall, he met the beautiful and gifted Vita Sackville-West, only child of Lord and Lady Sackville, and in 1913 they were married. Both Harold and Vita, as it happened, were bisexual, and they accepted each other's extramarital affairs. The author gives detailed accounts of the Nicolson's "Sapphism" (Harold Nicolson's euphemism), first disclosed in Nigel Nicolson's sensational *Portrait of A Marriage* (1973), but he stresses their cerebral and emotional compatibility. The couple had considerable literary talent, were prolific writers, and became intimate friends of the Bloomsbury group.

Lees-Milne, in tracing Nicolson's twenty-year career as a diplomat, is often superficial, biased, and even occasionally mistaken factually (see pp. 50, 68, 73–74, 91, 114, 115, 181, 194, 253, 338), but he gives graphic descriptions of the important public figures Nicolson met and the fascinating events he witnessed. Nicolson's initial diplomatic appointments were at Madrid (1911) and Constantinople (1912–14). He spent the war years at the Foreign Office working mainly on Balkan and related problems; was a junior adviser to the British delegation to the Paris Peace Conference, which he bitterly criticized in his *Peacemaking, 1919* (1933); helped Sir Eric Drummond organize the secretariat of the League of Nations; and in 1922, after the Chanak crisis, accompanied Lord Curzon to Lausanne to help arrange a new peace treaty with Turkey. He later praised the foreign secretary, in *Curzon: The Last Phase, 1919–1925* (1934), for his "superb handling" of the negotiations. After the death of Sir Eyre Crowe in 1925, and his replacement by a less friendly permanent under-secretary, Nicolson was appointed counsellor at Teheran. Then, while acting as chargé d'affaires, he wrote an impolitic memorandum criticizing the Foreign Office's entire Middle Eastern policy. Some months later, now demoted to first secretary, he accepted a post with the embassy at Berlin but, for professional and family reasons, grew increasingly dissatisfied with diplomatic life. Lees-Milne ends his first volume with Nicolson's decision in 1929 to resign and begin a new career as a columnist with Lord Beaverbrook's *Evening Standard*.

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W. J. READER. *Bowater: A History*. New York: Cambridge University Press. 1981. Pp. xv, 426. \$49.50.

This book describes the development of a small paper merchant's business, founded in London in 1881, into the largest manufacturer of newsprint in both Britain and the United States. This remarkable transformation was achieved through a series of dramatic events, including a takeover by Bowater in 1936 of a company with assets valued at 40 percent more than its own, conflicts and intrigues with several British press barons and the Bank of England, and a vigorous campaign of expansion from the late 1930s on into manufacturing and marketing in North America, the region where the Bowater empire is currently strongest.

There are company histories and company histories. The worst variety of this genre are glossy, uncritical public relations handouts. W. J. Reader's book, however, represents the very best kind of company history. It is a piece of distinguished scholarship written in highly readable prose. The book is based on a wide range of primary and secondary sources, all of which are fully documented. Although the study was commissioned by Bowater, the author adopts a rigorously critical approach to both decisions and personalities when appropriate. Thus the founder of the firm, W. V. Bowater, is described as "thoroughly unpleasant . . . hard-drinking, bad-tempered and tyrannical" (p. 7).

Among the virtues of this book is that Bowater's history is set firmly in its business and historical context. A great deal is said in the book about the publishing business and about other firms and industries with which Bowater came into contact. Moreover Reader, who has already written major scholarly histories of other leading British firms such as ICI and Metal Box, uses his unparalleled knowledge of British business history to provide comparisons between Bowater's history and that of other companies.

The information contained in this book has bearings on many issues of current interest to business and other historians on both sides of the Atlantic. Students of the growth of multinational business, of the role of foreign capital in the United States, of the development of managerial structures within firms, and of the business strategies of large oligopolistic companies all have much to learn from this book. Above all, however, this history provides an extremely lucid picture of the role in business of the individual entrepreneur. One man, Eric Bowater, is "the hero of the book" (p. 343). His decisions exercised a fundamental influence on the growth of the firm, usually for the better but sometimes for the worse. It was Eric Bowater's fascination with the potential of the United States, for example, that was the driving force behind the company's expansion into North America. This case study of an almost classical Schumpeterian entrepreneur provides a clear antidote to those economists and their fellow travelers in the historical profession who seek to

explain business behavior in terms of econometric and mathematical abstractions.

In short, this study is an important addition to twentieth-century Anglo-American business history. The book, which is excellently illustrated, is also a good read.

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I. D. MCFARLANE. *Buchanan*. London: Duckworth; distributed by Biblio Distribution Center, Totowa, N.J. 1981. Pp. xvii, 574. \$67.50.

The appearance of the first full biography of George Buchanan since Hume Brown's some eighty-five years ago is necessarily an exciting event. Ian D. McFarlane's *Buchanan* becomes all the more welcome as its author is intimate with the Neo-Latin culture of sixteenth-century France that nourished the great Scottish humanist for the greater part of his adult life. Only such a specialist is ever likely to integrate the growing number of significant specialized studies of Buchanan's thought into a cohesive understanding of this exceptional and wide-ranging mind. Such a promising and important undertaking can surely warrant so massive a volume—and, *just conceivably*, even so massive a price.

Unfortunately, the result has been an old-fashioned and somewhat disappointing biography. McFarlane appears to have seen his task more as filling in the blank years of Buchanan's life than as reinterpreting his mind. His study goes on at enormous length about whom Buchanan knew, or might have known, or could have known. This focus on whom, what, and where induces McFarlane to speculate endlessly about the possibilities—without much resolution. We confront a great list of names but little purpose. The extant manuscripts of Buchanan's writings are examined in exhaustive detail, but the works themselves are summarized rather than analyzed, and in the end McFarlane's painstaking efforts do not significantly further our understanding of them. To be sure, the biography does contain much new information, but it does little to alter the traditional picture: Buchanan emerges as an evangelical humanist rather than a militant reformer, whose Calvinism only developed as a result of the Scottish political context to which he returned (probably for incidental reasons) late in life. Although friend to both John Knox and Andrew Melville (as well as Patrick Adamson) and despite serving as moderator of the Church of Scotland, Buchanan felt decidedly little attraction to clericalism, biblicism, or, for that matter, apocalypticism. None of this is new, and McFarlane appears unaware of the truly exciting insights about Buchanan produced by recent scholarship. For McFarlane, Bu-

chanan's mind was broad but unoriginal. He has nothing to say about Buchanan the Stoic philosopher or about Buchanan and the theory of state. McFarlane meticulously traces the printing history of Buchanan's works, but he tells us nothing of how Buchanan actually shaped the minds of men.

The language of this biography is curiously precious in a way at least as old-fashioned as the work's conception and scope. For McFarlane religious repression and obscurantism become "backwoods-manship" (p. 403), education at Bordeaux was "in the swim" (p. 80), Buchanan saw the Franciscans as "the niggers in the woodpile" (p. 145), "had the Gaelic" (p. 19), and at least twice "crossed his Rubicon" (pp. 322, 488). Even in inverted commas it is misleading to speak of Buchanan's "'democratic' view of kingship" (p. 387, also pp. 168, 388, 394, 404, 409, 419, 482) or of "the indispensable role of a middle class" in Buchanan's and Aristotle's political theory (p. 401). No less quaint is the pseudopresent that McFarlane frequently adopts when speaking of events that occurred four centuries ago.

McFarlane hopes to reissue his *Buchanan* after the quartercentenary commemoration of the humanist's death and incorporate the new information about Buchanan that will appear in publications intended to mark that occasion (p. x). Any reissue should contain a good deal more than that, for fill-in-the-blank biography is long and justifiably outdated.

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R. A. CAGE. *The Scottish Poor Law, 1745–1845*. Edinburgh: Scottish Academic Press; distributed by Columbia University Press, New York. 1981. Pp. v, 180. \$20.00.

Six months of teaching British history in the United States have taught me to be more discriminating in the use of such terms as "English" or "British." Too casual a use of these ignores the existence of Scotland, or, worse still, assumes that most aspects of its historical development were shared with its southern neighbor. In many ways, not least in legal and religious institutions, Scotland was different. Failure to recognize this not only distorts British history, but leads to much of the excellent work in Scottish social and economic history produced over the past two or three decades being overlooked.

The poor law was one important sociolegal institution in which the differences of the two nations were marked. Since the appearance of the Webbs' massive study of English local government in the late 1920s, the English poor laws, old and new, have been much studied and discussed. By contrast, while some valuable essays on particular aspects of the

Scottish poor law have appeared of late, the standard work has remained Sir George Nicholls, *History of the Scotch Poor Law*, first published in 1856 and reprinted in 1967.

R. A. Cage, a graduate of the Checkland stable at Glasgow, has supplied the need for a more modern account with an informative study that is a model of brevity and compression. Beginning with a survey of Scottish poor relief legislation up to 1845, he then analyzes the workings of the pre-1845 system in both its rural and its urban contexts. He next discusses briefly the role of the private charities with which the official poor law system was so closely associated. From this, he hastens on to a comparison with the English poor law, and then to a critical account of the practical work of that influential Scots philosopher of philanthropy, Thomas Chalmers. In his penultimate chapter, Cage discusses the debate on the poor laws that raged in Scotland, as in England, once the Napoleonic wars ended and the economic and social consequences of peace began to be sharply felt. Finally he deals with the commission of enquiry of 1844 and its report, which provided the basis for the act of 1845. Ten years after England, Scotland received its new poor law, providing a more coherent, centrally supervised system of administration but few answers to the social questions posed by industrialization and urbanization.

Cage's work reveals the misinterpretations (some of them deliberate) of the Scots system by the English and vice versa. Poor law reformers in England viewed with admiration a system that they proclaimed (quite falsely) denied relief to the able-bodied unemployed and stressed the virtues of self help. Scots like Chalmers saw the English poor law as corrupt and enfeebled, thus proving the inherent viciousness of all statutory systems of relief. Cage's book should protect modern scholars from such errors, as well as showing that there is more to Scottish history than kilts, bagpipes, and Bonnie Prince Charlie.

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C. DUNCAN RICE. *The Scots Abolitionists, 1833–1861*. Baton Rouge: Louisiana State University Press. 1981. Pp. xii, 221. Cloth \$27.50, paper \$10.95.

British abolitionists were not content with the achievement of emancipation in the British West Indies in 1833. They turned immediately to the problem of slavery in the American South. The interaction between English and American abolitionists has been traced by Betty Fladeland in *Men*

and *Brothers*, published by the University of Illinois Press in 1972. Now C. Duncan Rice, a native Scot and an Edinburgh Ph.D. who is dean of Hamilton College, has traced the relation of Scottish men and women to the antislavery crusade in the United States.

After a very brief discussion of Scottish abolitionism before 1833, which he promises to treat fully in a later book, Rice proceeds to an analysis of the organization and membership of the Edinburgh and Glasgow Emancipation Societies, through which the bulk of later Scottish abolitionist effort was channeled. The members were largely middle class and represented chiefly the dissenting churches. For leadership they looked mainly to the radical English abolitionist George Thompson, who worked closely with William Lloyd Garrison. Scots enthusiastically supported Thompson's American tour in 1834–35. Rice observes that this tour not only produced few converts to abolitionism but actually increased American hostility to the cause, Thompson being regarded as "an infamous foreign scoundrel" (p. 70). The newspaper clippings and placards Thompson brought back with him, however, were effectively used to whip up further antislavery sentiment in Great Britain.

British as well as American abolitionism was struck a severe blow by the controversy over the role of women and over political action that culminated in the 1840 schism in the American Anti-Slavery Society and in the fight over denial of seats to female delegates to the World's Anti-Slavery Convention held in London the same year. Scottish abolitionism was further shaken as a result of the visit of John Anderson Collins, an American Garrisonian, in late 1840 and early 1841. A few years later more internecine warfare developed over the acceptance of financial contributions from Presbyterian churches in the slaveholding states of the American South to the newly organized Free Church of Scotland. Several prominent American abolitionists—Garrison, Frederick Douglass, and Henry C. Wright—traveled to Scotland to promote a fruitless campaign to "Send Back the Money."

Scottish abolitionism enjoyed a substantial revival when Harriet Beecher Stowe came to visit in 1853. She appealed particularly to members of the aristocracy, who had not been reached by the societies that had been formed in the 1830s. During the Civil War, the author notes, the Scots reunited in a vigorous campaign first to speed emancipation and then to send aid to the freedmen. He chooses to end his study in 1861, however, out of deference to Helen M. Finnie, the author of a Ph.D. dissertation on "Scottish Attitudes Towards American Reconstruction, 1865–1877," completed at the University of Edinburgh in 1975. Rice's book is a model monograph, thoroughly documented and well writ-

ten. It is a valuable addition to scholarly literature on the antislavery movement.

IRA V. BROWN
Pennsylvania State University

L. M. CULLEN. *The Emergence of Modern Ireland, 1600–1900*. New York: Holmes and Meier. 1981. Pp. 292. \$30.00.

In analyzing the economic and social forces that generated modern Ireland, L. M. Cullen discusses a population growth resulting from early marriages, increasing birth and retreating death rates, the consequence of a more varied diet, a decline in epidemic diseases, and an immigration that for much of the seventeenth century probably made Ireland the recipient of "the largest stream of migration in the entire North Atlantic world" (p. 84). Many of the planters in Leinster, Munster, and Connacht were the younger sons of the English gentry and yeoman farmer classes. Many Scots settled in culturally backward, economically underdeveloped, and underpopulated Ulster. Immigrants built and then reconstructed villages on or close to their estates, modernized the agrarian economy, and added grains, meat, vegetables, and fruit to the simple Irish dairy diet. But the population boom they augmented put much pressure on the Irish agrarian economy and limited opportunity, driving many of the younger sons of the Catholic aristocracy and gentry, already troubled by land confiscation and Penal Laws, to the Continent or the colonies. It also discouraged use of the Irish language and increased the importance of the potato.

Cullen claims that in magnifying the significance of the potato, historians have distorted the reality of the 1840s Famine. He says that strong and middle tenant farmers were eating meat, fish, grains, and vegetables on the eve of the Famine. Therefore, the catastrophe was more localized than similar eighteenth-century occurrences. Only the destitute—of course there were many—in the Southwest and West ate only potatoes and perished with them.

Cullen's book is packed with information and interpretations on a variety of subjects including the relationship between the economy and Irish hospitality, culinary arts, religious and cultural tensions, and conflicts between Catholics and Protestants. He attributes reforms in nineteenth-century Catholicism to catching up with the Council of Trent and not Jansenism. He argues that 1798 in Wexford was more of a clash between marginal Catholic and Protestant gentry than a peasant revolt. He points to Catholic abandonment of the native language as evidence that the Irish have not been tradition minded. He claims that the Irish-Ireland movement has been an identity search rather than a preservation of tradition.

Since Irish economic and social history has suf-

fered from neglect, Cullen has made a contribution. But while offering interesting and provocative interpretations, he seems to promise more than he delivers. Cullen obscures his main themes by poor organization of material, excessive qualification, and dull presentation. Except for comments on the increased consumption of tea, whiskey, and commercial bread, he tells little about post-Famine Ireland. And he does not really enlighten the reader about the lifestyles of the Irish Catholic peasant majority—some comfortable strong and middle farmers and many impoverished small tenants and agricultural laborers. Cullen's confusing observations on diet and the Famine still leave the potato as an Irish historical mystery and demonstrate that quantitative methodology offers little insight into the misery of destitution. His study does not appear to affect seriously present consensus views of Irish history.

LAWRENCE J. MCCAFFREY
Loyola University of Chicago

DONALD HARMAN AKENSON. *A Protestant in Purgatory: Richard Whately, Archbishop of Dublin*. (Conference on British Studies Biography Series, new series, number 2.) Hamden, Conn.: Archon, for the Conference on British Studies and Indiana University at South Bend. 1981. Pp. xiii, 276. \$19.50.

Donald Harman Akenson's book is the second in a new series of biographies sponsored by the Conference on British Studies that give emphasis to personality. It provides a lively portrait of one of those important secondary figures whose careers illuminate historical change, in this instance the declining influence of the established church in Victorian Ireland. Like many Anglican bishops, Whately began as an academic, first as a fellow of Oriel College, later as Drummond Professor of Political Economy. At Oxford he knew a number of outstanding young men, including Nassau Senior, John Keble, and John Henry Newman. In 1819 he published a clever satire, *Historical Doubts Relating to Napoleon*, in which he used the arguments David Hume had employed to reject Christian miracles to prove that Napoleon had never existed. More solid evidence of Whately's ability were his treatises on *Logic* (1826) and on *Rhetoric* (1828). Whately's religion was more intellectual than emotional, yet at the same time pragmatic. In *Church and Society in England 1770–1970* (1976) E. R. Norman observed that in the early nineteenth century progressive churchmen embraced classical economics. In addition to lecturing on political economy, Whately published a primer to introduce children to its virtues and to reconcile it with Christian morality. He laid stress on individualism but was generous toward the poor and later came to recognize that Ireland had need for a public works

program to deal with unemployment. He advocated religious toleration and supported Catholic emancipation.

The Whigs, who appointed Whately archbishop in 1831, hoped that his liberal views would assist in diffusing religious conflict in Ireland and made him a member of the National Education Board. As bishop, Whately sought to raise the level of religious education among laity and clergy. He opposed both the Tractarians and the Evangelical party, deploring the latter's efforts to proselytize among Catholics. As a commissioner for education, he worked successfully with Daniel Murray, the Catholic archbishop of Dublin, to establish a nonsectarian school system. It proved a hopeless task as most schools came under the influence of local Protestant or Catholic clergy. When Paul Cullen replaced Murray in 1852, Whately was soon drawn into a dispute that ended with his resignation from the commission. The remaining decade of his life was largely a rear-guard action against his critics, family losses, and poor health.

Akenson, author of *The Irish Educational Experiment* (1970), as well as a study of the Church of Ireland, is well qualified to explain the complex problems confronting Whately both in his episcopal duties and in national affairs. Always the Oxford don, the bishop's candor and self-assurance did not endear him to his clergy, mostly Trinity men, Tories, and Evangelicals. In dealing with Catholics he was even more handicapped; the new spirit of Irish nationalism combined with ultramontane discipline made men like Cullen reject almost everything Whately stood for. Like most good biographers, Akenson is a protagonist for his subject, and he is more critical of Cullen than scholars like E. R. Norman or Emmet Larkin. Nevertheless, he recognizes the salient reason for Whately's failure. Even a man of ability and determination could do little to reform and revive the established church in post-Famine Ireland.

FRANCIS G. JAMES
Tulane University

IAIN A. CAMERON. *Crime and Repression in the Auvergne and the Guyenne, 1720–1790*. New York: Cambridge University Press. 1981. Pp. xvi, 283. \$37.50.

Iain A. Cameron's work is our first detailed study of the operation of Europe's earliest national police force, the French Maréchaussée. Founded in the Middle Ages as a military police force, the Maréchaussée's police and judicial powers were extended by the monarchy in the sixteenth century to encompass the civilian population. As a police force the Maréchaussée apprehended deserters, patrolled highways, supervised foreigners, maintained order at country fairs and markets, aided in executing court orders, and made arrests both in crimes threatening the public order and at the behest of the

Intendant. As a judicial institution the Maréchaussée operated the *prévôtés*, courts presided over by police officials whose decisions were not subject to appeal. *Prévôtal* competence extended to cases of highway robbery, burglary, possession of counterfeit coins, riot, and all offenses committed by vagabonds, beggars, and recidivists.

Cameron selected two very different provinces, the rugged and impoverished Auvergne and the prosperous Guyenne, in which to study the Maréchaussée from its reform in 1720 to its incorporation into the Gendarmerie Nationale in 1790. Cameron's extensive research in municipal and departmental archives as well as in the Archives Nationales and the Archives Historiques de la Guerre permits him to identify the many problems of eighteenth-century police work. Investigative methods were primitive. Police manpower was inadequate and extraordinary orders, like that of the military governor of the Guyenne requiring the disarmament of *roturiers*, distracted policemen from routine duties. Low pay, often in arrears, drove some policemen to moonlighting or to minor acts of corruption. But Cameron's research also demonstrates gradual improvement in police service in the last decades of the Old Regime resulting in a growing willingness of civilians to cooperate with the Maréchaussée.

Cameron's analysis of the judicial function of the Maréchaussée yields some significant conclusions. The *prévôtés* long have been criticized for their brutal, summary justice, but based on the records of the *prévôté* in Périgueux Cameron concludes that judges of this tribunal weighed evidence carefully, seldom tortured prisoners, and demonstrated a growing leniency in their sentences. Cameron's treatment of the crimes judged by the *prévôtés* sustains, for the Périgueux area though not for the Auvergne, the hypothesis that eighteenth-century France witnessed an evolution in its crime pattern from one dominated by violence to one characterized by property crime. This conclusion must, however, remain tentative, as Cameron recognizes. The restricted competence of the *prévôtés* meant that many crimes were not heard by the police courts, but by ordinary courts. A fuller picture of rural crime thus requires that Cameron's work be supplemented by study of the proceedings of ordinary criminal courts. Cameron's monograph, nonetheless, is an important contribution to a growing literature on the history of crime and to our knowledge of the institutional and judicial structure of Old Regime France.

JULIUS R. RUFF
Marquette University

NICOLE CASTAN. *Les criminels de Languedoc: Les exigences d'ordre et les voies du ressentiment dans une société pré-révolutionnaire, 1750–1790*. (Publication de

l'Université de Toulouse-Le Mirail, series A, number 47.) Toulouse: Association des Publications de l'Université de Toulouse-Le Mirail. 1980. Pp. viii, 362.

In August 1781, Raymond Bessoles shot his youngest brother, Antoine, and stuffed a handkerchief in his mouth, saying, "You have eaten enough choice bits, here is your last." Raymond was devastated by his father's decision to use his "testamentary liberty," an enormous source of patriarchal power, to pass on the patrimony to the "doux et brave Antoine." The Bessoles' Rouergat neighbors blamed the whole affair on the father. Although he had the legal right to do what he did, custom and tradition dictated the right of the eldest, which he transgressed, "merely out of affection."

This tale takes us to the heart of what Nicole Castan has accomplished in this excellent book. Criminal court records provide a wealth of detail rarely found elsewhere about the lives and yearnings of ordinary people. Through the study of its crime, Castan has brought to light the inner dynamics and changing face of an entire society. The *ressort* of the Parlement de Toulouse (Languedoc-Guienne-Gascogne) experienced in these years the wrenching impact of capitalist economic growth, seigneurial reaction, increasing administrative intervention, and, above all, rapid rural population growth, which not only heightened conflict in the countryside but also created an "uprooted" migrant population that terrorized the towns.

Castan distinguishes between the "commonplace" criminality characteristic of the region historically and that which marked the prerevolutionary crisis. Much of the former derived from the high sense of honor, personal and familial, associated with one's place in society. And, for those who had a place, village society in the Midi was notably egalitarian. Hence, in conflict, came the refrain, "You are no more than I, you are just a mere mortal," usually followed by a crescendo of oaths and blows.

But the pressures of the later eighteenth century "shook the edifice in its entirety." Each chapter moves from the normal to the extraordinary, while the crisis became deepest. On the way we feast on brilliant analysis and illuminating detail. The endless conflicts over property, the right of transit, and the use of commons; the seigneurial reaction and the revolt against it; the struggles over municipal power and rank; the defense of the patriarchal family order, of chastity and virility, of inheritance rights and the patrimony appear in all of their complexity and growing acerbity. Beyond lay the criminal menace of those without place, the legions of the poor, who created a permanent state of fear long before the *grande peur*.

Urban society was more revolutionary still. The battle for space, in this glutted environment, was the root of much petty crime. The battle for place, for "insertion sociale," was ferocious in a social structure unable to absorb many new people. It was revolutionary, too, it seems, in its attitudes toward love. Illegitimate "pregnancy trials" reveal strong currents of romantic love, direct testimony of the sort that Edward Shorter would have loved to have. Castan then presents the final balance sheet: a crushing weight of crimes against property and a relative decline of crimes against persons. She concludes with a discussion of collective violence in the 1780s that simply flowed into the Revolution. Regrettably she does not analyze the social and geographical origins of participants, thus missing the chance to assess the differences between collective and individual rebellion against the law.

This reservation aside, Castan has written a wholly satisfying study: a profound analysis of popular culture and a skilled dissection of a society hurtling toward revolution.

CHRISTOPHER H. JOHNSON
Wayne State University

PATRICE HIGONNET. *Class, Ideology, and the Rights of Nobles during the French Revolution*. New York: Clarendon Press of Oxford University Press. 1981. Pp. xvii, 358. \$55.00.

Patrice Higonnet has used the subject of the rights of nobles to discuss the role of class and ideology in the development of revolutionary politics. His basic thesis that "When political circumstances forced it to choose between property and community, the bourgeoisie chose an entente with the 'peles' in the name of community in preference to an entente with the nobles in the name of property" (p. 258) may not appear controversial, but his application of it will undoubtedly raise strong objections.

Higonnet, who assigns ideology an autonomous and powerful role, believes both that individualism (read property) and community were the basis of bourgeois ideology in the 1780s and that the contradictions in these universalisms were unrecognized in 1789. Consequently it took a decade of political events to clarify the situation. The process was made haltingly, one group at a time, with the nobles being a key focus of the transformation. One fascinating and controversial conclusion by Higonnet is that the Feuillants were the first and the Jacobins the last group to recognize the true social reality.

For Higonnet, the revolutionary decade had four periods. Initially bourgeois universalisms were pursued with the aid of individual nobles, while the corporate nobility was condemned as a parasite that violated the bourgeois sense of community. By the

summer of 1791 the first set of revolutionary bourgeois, the Feuillants, understood that the men of property, including nobles, had to stand together to withstand the threat of universal egalitarianism. The Girondins, however, curiously defined as "well-connected modernizers," pushed the revolution forward by opportunistically using antinobility to win popular support. By the fall of 1793 both the Girondins and the sans-culottes had seen the danger and sham of antinobility, but Robespierre and Saint-Just reacted to the stalled revolution by using an abstraction, the Republic of Virtue where nobles personified corruption, to obscure the contradiction between property and egalitarianism. Under the Directory these three stages of antinobility—individual, opportunistic, and abstract—were repeated as farce, yet with the effect of demonstrating that men of property must put community aside, which they did by supporting Bonaparte and his reintegration of nobles into society.

Despite my agreement with Higonnet's general view of the revolution, I am troubled by a number of specifics. The role of war is downplayed almost to the point of total neglect. Not only does this remove an important ingredient of antinobility, but it makes the analogy Higonnet draws to antisemitism in the 1930s misleading. More attention to the issue of war would also have focused the discussion of noble émigrés, an issue along with several others that deserved more attention than treatment in an appendix. Despite numerous and lengthy quotations, I often felt a lack of evidence to support the heavy use of logic, and for the period of the Directory the clarity of the logic broke down.

For Higonnet, the French economy and the ideology of the bourgeoisie-nobility were both mixed, and under the pressure of revolutionary events the liberal nobles, then the bourgeois notables, and finally the provincial bourgeois realized their true interest in property. I found these ideas provocative; others may react much more strongly.

THOMAS D. BECK
Chapman College

ROBERT J. SMITH. *The École Normale Supérieure and the Third Republic*. Albany: State University of New York Press. 1982. Pp. vii, 201. Cloth \$39.00, paper \$12.95.

The role of the *École normale* in the political and intellectual life of the Third Republic is familiar to historians in a general way, but in this concise and well-rounded study Robert J. Smith brings it into scholarly focus. There are chapters on the system of entry to the school, on the social composition of its students, on its internal life (happily sparing us too much detail of *normalien* slang and folklore, on

which the works of alumni tend to dwell), on politics at the school itself, and on its wider relations with the Republic. This last chapter is especially useful, as is that on the students' social origins and careers. The school sustained a powerful myth of recruitment from "the deep beds of worker and rural democracy" (Director Georges Perrot in 1899, cited p. 28), but Smith shows that the typical student was from a family in state service in a large town; secondary teachers were more common than elementary ones among the parents, and both peasants and workers were rare. Still, meritocracy was real enough to make the school a liberal stronghold.

Smith argues that *normaliens* were a "marginal elite" before 1870, but came into their own under the Third Republic because of its need for ideologues and organizers of the national educational system. The period of the celebrated librarian Lucien Herr, of the Dreyfus affair, and of Péguy naturally comes under close examination. Smith concludes that although the school was Dreyfusard, only a minority became Socialists, and he suggests that the school's real center of gravity shifted from Gambettism in the Republic's early years to the kind of radicalism represented by Herriot. The socialism of Jaurès and Blum could be fitted into this pattern, but only a few individuals were attracted to communism, to fascism, or (despite Pompidou) to technocratic Gaullism. In recent years, Smith concludes, the school has lost its wider political role. But perhaps that is not the end of the story, for elite institutions have shown remarkable tenacity in France, and Smith shows how the *École normale* survived a reform in 1903–04 designed to integrate it with the Sorbonne.

Smith's book naturally invites comparison with Terry Shinn's recent study of the *École polytechnique*. His discussion of the school's relations with the wider society is more subtle and wide-ranging than Shinn's, but Shinn perhaps gives a clearer picture of the intellectual formation that students received, and Smith could well have expanded his treatment of the classical and philosophical traditions that formed the *esprit normalien*. He also says little about the scientific section, always less prominent in accounts of the school. The scholarly and scientific attitudes learned at the school percolated throughout the French educational system, supporting that intellectual liberalism that accorded so well with the individualist democracy of the Third Republic, but less easily with the ideologies and collectivist pressures of the twentieth century.

R. D. ANDERSON
University of Edinburgh

MAURICE VAÏSSE. *Sécurité d'abord: La politique française en matière de désarmement, 9 décembre 1930–17 avril 1934*. Foreword by JEAN-BAPTISTE DUROSSELLE. (Pub-

lications de la Sorbonne, Série Internationale, number 18.) Paris: A. Pédone. 1981. Pp. xv, 653.

This is an outstanding example of academic history. The study of French foreign policy and the issue of disarmament in the early 1930s is thoroughly, even painstakingly researched, intelligently conceived and organized, highly disciplined in its literary style, and excellently balanced and reasonable in its analysis. Maurice Vaïsse has produced both a specialized monograph in French interwar diplomatic-military history and an important contribution to the understanding of relations between major domestic affairs and external policy in the late Third Republic. The sad details of this history are essential reading for any student who looks into the causes of declining French power.

Vaïsse conducts his exploration and assessment on several levels. First, French security is viewed as it was determined in this complex democratic environment of 1930–34. In one of his most vivid achievements, the author dissects the disarmament and antidisarmament camps through detailed analysis of the numerous political parties, unions, the press, and the elected deputies of 1932. The stellar contribution made by Vaïsse is his fascinating portrait of such diversified groups as the U.N.O.R. (l'Union Nationale des Officiers de Reserve), les Jeunesses Patriotes, and le Comité Dupleix on one side and the Ligue des Droits de l'Homme, la Ligue des Anciens Combattants Pacifistes, and Le Mouvement E. N. (Europe Nouvelle) on the opposite. The confusion and near anarchy of objectives and means of these pressure groups leave the reader enlightened but dismayed by the infighting of French policy formation.

The second section, which explores the issues of the disarmament debate, gets to the heart of civil-military relations. The structure within which the experts (technical, legal, and diplomatic) functioned and carried on a discourse is detailed with fine, pointed strokes. Louis Aubert, Pierre Cot, Robert Jacomet, and René Massigli become the proponents of various positions, as do the military men Delauze, Requin, and Serriguy. The role of these expert advisers is finally translated in the sections dealing with the responsible policymakers. Here the Vaïsse book clearly is superior to the general Wheeler-Bennett and Noel-Baker treatments or the French-oriented efforts of Jacques Minart. The descriptions and evaluations of the "men of power" (Herriot, Tardieu, Daladier, Paul-Boncour, and Weygand) on such topics as military strategy, preparedness, and organization are worth reading even if one is not a specialist in modern French history or defense matters.

This reviewer is impressed by not just the quantity of primary sources cited but by their extensive use

and value. Admittedly, Vaïsse is dealing with a period of time and an issue that the few tomes of the DDF (Documents diplomatiques français) that have been completed do cover rather comprehensively. But, in the fashion of a super *thèse de doctorat*, Vaïsse utilizes numerous state papers (especially of the Ministries of Air, Sea, and War), League of Nations materials, private papers (Dumesnil, Paul-Boncour, de Brion, and Tardieu), those of the aforementioned pressure organizations, and a collection of interviews.

This work must stand in the very front rank of books dealing with the modern French experience in government and foreign policy. It is the best available historical research and analysis of the "inputs" from below and outside (concerning the major military questions) as they influenced the French leadership. The whole complex of forces within the nation is demonstrated with an erudition that goes beyond the copious research. The treatment of German, British, Italian, Belgian, and American stances *vis-à-vis* disarmament is substantial, yet this is a national study. Lessons can be and should be drawn from this work, but they are subtly expressed, not driven home with force. In sum, Vaïsse has crafted the richest, best-documented one-volume study of the interwar French disarmament controversy and, by the way, an indispensable work for every student of this once again contemporary problem.

PIERRE-HENRI LAURENT
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Fletcher School of Law and Diplomacy

ALAN F. WILT. *The French Riviera Campaign of August 1944*. Carbondale: Southern Illinois University Press. 1981. Pp. 208. \$15.00.

There were no topless attractions at Saint Tropez in 1944. On August 15 the Allies under Mediterranean Command launched a major land-air assault against German positions on the French Riviera. Saint Tropez was at the center of the amphibious operations, which entailed a series of landings on beaches to the east of Toulon and to the west of Cannes. Within the space of one month, this assault force of roughly three divisions had achieved its tactical objective—the liberation of Toulon and Marseilles—and had executed a stunning offensive drive northeast toward Belfort. Assured of overwhelming naval and air superiority, armed with precise intelligence from Resistance sources and from Ultra, and confronted by an ill-prepared and ill-equipped enemy, this American-French force was able to record a spectacularly successful campaign. Alan F. Wilt's lively account of the operation ends in September 1944 with the link-up of this Operation

Dragoon force with that of General Patton's break-out army from Normandy.

Wilt has chosen an interesting and, arguably, important subject. The strength of his book lies in the detailed recollection of the campaign itself. In this respect, he brings to our attention much precise information that hitherto has been confined to official, but unpublished, campaign histories. It is a story worth being told, and to the extent that it is a story, a narrative, the book exposes the planning and execution of the campaign with commendable clarity.

As an analytical work, the book is less satisfying. First, while the push for Dragoon consistently came from the American Joint Chiefs of Staff, at no point does Wilt adequately explain the reasoning behind that unshakable determination. Second, on two occasions his cursory explanations of motive do touch upon French thinking and sensibilities (pp. 46, 55–56). These brief references only serve to remind us, however, that the French role in all of this is much neglected. There is more to this than the absence of French archival material. One is skeptical, for example, of the notion that this Franco-American operation was conceived within the exclusive confines of an Anglo-American dialogue. In short, the role that the Free French government played in this invasion of France is largely unexplored. Consequently there is the related uncertainty of knowing how any number of considerations relating to France and the French may have influenced the respective and discordant positions of London and Washington.

Third, no sooner had the Riviera campaign ended on a successful tactical note than the British and Americans resumed their debate over the strategic value of what had been done. The British were still asking whether Dragoon was not a disguised blessing for German forces in Italy and whether Germany would not draw substantial benefits from having been forced to regroup its armies in eastern France around the defensive stronghold of Belfort. In his ten-page "Assessment" chapter, Wilt does nothing to resolve this debate. For him it boils down to the fact that the Americans wanted this campaign and that, in 1944, they were getting their own way. This may be true, but by treating as "academic" this debate over strategy Wilt refuses to instruct us either on the wisdom of the campaign in principle or, in practice, on its overall strategic efficacy within the broader context of the war in Europe.

ROBERT J. YOUNG
University of Winnipeg

RONALD GOETSCHEL. *Meir Ibn Gabbay: Le discours de la Kabbale espagnole*. Louvain: Peeters. 1981. Pp. 565. 1,860 F.

Half a century ago most scholars in Jewish history would have found this book unintelligible or wrong-headed in the extreme. But thanks principally to the late Gershom Scholem and scholars like Georges Vajda (to whom this book is dedicated), we have learned to see that mysticism as embodied in the Kabbalah is as normative to Judaism as the rationalist intellectual tradition.

Roland Goetschel's book contains few surprises and is essentially à la Scholem. Such dependence is less a fault than a fact of life for the student of Jewish mysticism, who must for a long time to come work as a disciple, like it or not, within the framework established by Scholem. Since Ibn Gabbay was not an original thinker but a synthesizer, his writings mirror in micro the larger world of Kabbalah as depicted by Scholem. Goetschel also labors under the handicap that everything known of Ibn Gabbay's biography can be stated in a sentence: He was born in 1480, exiled in 1492, lived perhaps in the Ottoman empire, died in 1540 perhaps in Palestine. Thus Goetschel has to string many beads on meager biographical threads, imparting a more general character to his book.

Goetschel enters one caveat. Scholem posits that the 1492 expulsion was the dire event that effected a shift in cabalistic speculation toward that passionate messianism that culminated in Sabbatai Zevi; Goetschel argues that this "mutation" predates 1492 and stems from the Marrano trauma that began in 1391; the point remains moot since one's interpretation depends on two passages from Ibn Gabbay that, as so often in Kabbalah, contradict each other.

The one surprise here is the vehemence of Ibn Gabbay's refutation of the rationalist position as expounded by the "impious" Maimonides more than three centuries earlier! Ibn Gabbay's motto was "Look homeward, Angel," and he would make war on all outside influences: Greek science was "an emanation of Satan," as evil as "the donjon" of Islamic thought (pp. 85, 87). Outsiders, however, were drawn to Ibn Gabbay, most notably Christian cabalists. Such links, though a minor part of Goetschel's book, will be of great interest to the classroom teacher who is anxious to breach the walls of the ghetto of Jewish studies, so that general history will reflect some of the results of two generations of intensive Jewish scholarship—all too little of it has trickled down to syllabus, textbook, and lecture.

For historians the most important parts of the book are those segments that constitute a prolegomenon to Ibn Gabbay's three treatises: a pithy sketch of the Marrano chapter of Spanish history, an account that shows the full sweep of the Spanish Kabbalah's evolution from the thirteenth century forward; a penetrating linguistic analysis of Ibn Gabbay's texts; and an estimate of his influence and

the interest in him of his contemporaries and successors. The heart of the book is a classic *explication de texte*. Goetschel's mastery of the sources, especially the biblical and talmudic literature, must be the envy of anyone, particularly those who seek to attain it in middle life. Yet it is not clear to whom this book is addressed; the student of Ibn Gabbay will read his three works, thus rendering much of Goetschel's book superfluous; someone plunging into Kabbalah will probably be better served by taking up one of several other accounts.

FREDERICK M. SCHWEITZER
Manhattan College

STEPHEN HALICZER. *The Comuneros of Castile: The Forging of a Revolution, 1475–1521*. Madison: University of Wisconsin Press. 1981. Pp. ix, 305.

For most historians, the revolt of the *comuneros* marks the critical divide between medieval and modern Spain. The Castilian cities that rebelled against the authority of the emperor Charles V in 1520–21 sealed the fate of a society hitherto characterized by a vigorous urban life and a strong parliamentary tradition. Following their defeat at Villalar, royal absolutism triumphed in Castile. Within the broad outlines of this "liberal" interpretation of modern Spanish history—an interpretation by no means universally accepted—the seminal importance of the *comuneros* has been established since the early nineteenth century. Recently its centrality has been reaffirmed by the publication of major studies by Maravall (1963), Pérez (1970), and Gutiérrez Nieto (1973). None of these works, however, has been translated into English, and for a comprehensive treatment of the subject English readers have had to rely on Henry Latimer Seaver's very dated, albeit gripping, narrative, *The Great Revolt in Castile*.

With the appearance of Stephen Haliczer's *The Comuneros of Castile*, this limitation is removed. By drawing extensively on the works of his contemporaries, Haliczer brings his reader abreast of current research. But his book is more than a synthesis; his own archival research and unique emphasis allow him to make an important contribution to our understanding of the revolt.

Haliczer places greater emphasis on the long-term preconditions to revolution than on the short-term precipitants. Employing a functionalist sociological model, he attempts to prove the relationship between revolution and "profound structural change of a 'dialectical nature'" (p. 9). In Castile the changes involved the rise to prominence of the cities and their elite of aldermen and merchants; the emergence of a new aristocracy greedy for land, revenues, and jurisdictions; and the growth, particularly during the reign of Isabella (1474–1504), of a

centralizing monarchy. Inherently contradictory, these changes produced the conditions necessary for revolution as early as the 1490s. From that point the interplay of social, economic, and political forces worked to the disadvantage of the cities—especially those of central Castile. By favoring the aristocracy in its extension of the seigniorial regime, the Mesta and the exporters of raw wool, and the luxury manufacturing interests of Andalusia, Isabella drove the cities of the *meseta* into opposition. Ferdinand's continuation of these policies, then the uncertainties and abuses associated with Charles's accession and early government, transformed this opposition into open revolt.

Haliczer's thorough and well-crafted study places the revolt of the *comuneros* in new light. First, by locating its preconditions in the late fifteenth century, he not only makes Isabella bear some of the responsibility, thus qualifying her reputed genius as a state builder, but also reveals the workings of deeply rooted tensions within Castilian society. Second, by arguing that after their abortive revolution the cities still managed to wring major concessions from Charles V, he challenges orthodox opinion on the revolt's outcome. Here his thesis strongly resembles the argument in Charles Hendrick's unpublished Columbia University dissertation, "Charles V and the Cortes of Castile: Politics in Renaissance Spain," a source that Haliczer curiously fails to mention. As with any revision, there appear to be elements of overstatement. For example, Haliczer invites skepticism about the degree of Isabella's favoritism toward the great magnates by ignoring the political repercussions of her takeover of the military orders. By shifting the origins of the *comuneros* to the 1490s and by arguing for a reevaluation of the consequences, however, he ensures the continuation of a historical debate central to our understanding of the formation of modern Spain.

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EBERHARD STRAUB. *Pax et Imperium: Spaniens Kampf um seine Friedensordnung in Europa zwischen 1617 und 1635*. (Rechts- und Staatswissenschaftliche Veröffentlichungen der Görres-Gesellschaft, new series, number 31.) Paderborn: Ferdinand Schöningh. 1980. Pp. 490.

This *Habilitationsschrift* for the University of Munich is a major work, even though it is one-sided and often exasperating. Eberhard Straub sets out to explain Spanish policy in Central Europe from the Oñate Treaty (1617) to the Peace of Prague (1635). He argues that the best hope for Germany, and for Europe, at the time was peace and order under Spanish leadership, the *pax Austriaca* envisioned by

Olivares. Spanish policy was "imperial." French policy under Richelieu "imperialistisch" (pp. 234–35). The former looked to preserve an existing order without further conquest, the latter to upset this order through crisis and conflict for the aggrandizement of France. Spanish policy had its roots in the fifteenth century and developed under Charles V and Philip II, and it was inspired in part by the example of ancient Rome. Nor was it impossible to realize. Much recent writing on the social and economic decline of Spain, valuable as it is, has distorted the situation before 1640. Until then, Straub contends, Spain was clearly the most powerful nation in Europe.

Crucial to the maintenance of Spanish hegemony was the assertion of the Spanish position in Flanders and North Italy. This in turn was dependent on stability in Germany under a Habsburg emperor. Germany was to be an instrument of Spanish policy, in that Spain expected aid from the emperor and princes against the Dutch and against the French, especially in Italy. But cooperation with Madrid was also in the best interests of the emperor and princes. Only with the support of Spain could they maintain their position against an increasingly aggressive France, as was proved in the event by the weakened empire that emerged from the Peace of Westphalia. The villains of the story are Maximilian of Bavaria, the leader of the Catholic League, whose own ambitions consistently prevented him from recognizing the German community of interest with Spain, and Emperor Ferdinand II, who all too often sided with his cousin in Munich instead of his cousin in Madrid. They were responsible for the failure of Spanish policy.

On the basis of extensive primary research chiefly in Simancas and Madrid, Straub has effectively placed Spanish policy for the empire within the framework of Spain's imperial vision, and he has illuminated the efforts to implement it. He has shown, for example, that the principal purpose of the Oñate Treaty was to provide for the imperial succession, not to obtain greater security for the "Spanish Road" from Milan to the Low Countries. But his evaluation of Spanish policy and the opposition to it reads like an apology for Olivares. Straub has little understanding of why Maximilian and Ferdinand did not act as Spain expected; nor apparently did Olivares. To mention only two examples of many, Maximilian never pursued the imperial title for himself, as Straub claims. Generally speaking, apart from the disastrous Edict of Restitution, after 1628 Vienna was much more aware of the limits of Habsburg power than Madrid, as a reading of the reports of the imperial councilors shows. This brings us to the point of exasperation with Straub. He has ignored major published sources, not to mention unpublished ones, which would have com-

pelled him to modify his interpretation significantly. The multivolume *Briefe und Akten zur Geschichte des dreissigjährigen Krieges* is not even listed among his sources, nor are the many documentary collections dealing with the Wallenstein issue, nor the documents assembled by Konrad Repgen in his study of the papacy. Major secondary works have been overlooked, such as those of Auguste Leman, Geoffrey Parker, and Golo Mann. Moreover, it is unfortunate that this volume appeared too early to make use of the *Memoriales y Cartas del Conde Duque de Olivares*, published by J. H. Elliott and José de la Peña (1978–80). A good editor would have greatly benefited Straub by shortening his lengthy summaries of state documents and generally simplifying his style.

ROBERT BIRELEY

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W. TH. M. FRIJHOFF. *La société néerlandaise et ses gradués, 1575–1814: Une recherche sérielle sur le statut des intellectuels à partir des registres universitaires*. Summary in Dutch. Amsterdam: APA-Holland University Press. 1981. Pp. xvii, 422. f 80.

W. Th. M. Frijhoff's monograph is the model of the modern quantified thesis. As such, it incorporates both the strengths and weaknesses of that particular model. The work's major strength lies in its painstakingly systematic compilation of data. Its author has identified and enumerated to the best of his ability every university graduate from the northern Netherlands during the period under study. His computer-assisted project tells us where and what they studied and what their professional aspirations were. It provides an impressive overview. The work also contains some suggestive material that perhaps points the way for future study. For example, the pluralistic and increasingly cosmopolitan nature of the early modern Dutch Republic is evidenced by its acceptance and graduation of Jewish and Catholic students as well as its acceptance of foreign licenses and diplomas. This subject is surveyed almost exclusively with statistical data (pp. 51–55).

The form taken by the study thus raises some questions. An initial indication of the difficulty appears in the title, *La société néerlandaise et ses gradués, 1575–1814*. Frijhoff deals more successfully with the second part, telling us a great deal, numerically speaking, about those who were graduates of the universities of Leiden, Franeker, Groningen, Utrecht, and Harderwijk. But he confines his investigation to time periods, geographic areas, and universities for which he has a complete numerical series. This means that he virtually disregards universities such as Nijmegen for which his sample is incomplete. He concentrates, additionally, on three particular reference points for which his data are

most complete: place of origin of medical doctors practicing in Amsterdam, the schools from which these doctors graduated, and selected groups of students from Zutphen and North Brabant families. The question of how typical these three groups were is overlooked. More seriously, Frijhoff devotes only one complete page (pp. 37–38) to those who failed to graduate from university, but who either attended for a shorter period of time or traveled to several universities to study.

Finally, since Frijhoff's energies have been devoted to compiling and analyzing the numerical data, he does not tell us as much as we might wish about the particular individuals who make up his statistical sample. How did they, as individuals, interact with that "Dutch society" of the title, of which they were members? What impact did they have on Dutch social developments? The author concludes that they were influential, but he does not demonstrate how. Now that his numerical basis is substantiated, it is to be hoped that Frijhoff will go on to do that. His subject is of significance not only for a better understanding of early modern Dutch society, but also because of its insight into the enormous influence that the Dutch Republic had on educational development in early modern Europe.

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HERMAN BAKVIS. *Catholic Power in the Netherlands*. New York: McGill-Queen's University Press. 1981. Pp. xiii, 240. \$24.95.

The title of this book is perhaps a bit misleading, because its subject is not just Catholic power but the decline of the Catholic subculture in the Netherlands in the 1960s and 1970s. Herman Bakvis describes in detail how this subculture constituted at one time one of the most cohesive social groups in Western Europe until its rather sudden disintegration in recent years. Dutch Catholics inhabit the provinces of Limburg, Brabant, and Gelderland and represented at one time 35 to 40 percent of the total population. They were a barely tolerated minority in the Dutch Republic and were not fully emancipated until the nineteenth century. Regardless of their emancipation, Catholics preferred to isolate themselves and to remain a cohesive subculture. Their solidarity is best exemplified by their voting pattern and political behavior. For example, the Catholic People's party (*Katholieke Volkspartij*) received during the period 1918–63 between 29 and 32 percent of the total popular vote and, in 1963, 85 percent of the Catholic vote. As a result, the Catholic party participated in every cabinet except one between 1918 and 1963.

The main pillar of the Catholic party was the hierarchy of the church, especially the bishops who successfully enforced a measure of orthodoxy and

isolation from the mainstream of Dutch society. It was this hierarchy that kept the party together and instructed the faithful how to vote. Thus, Catholics tended to view voting for the Catholic party as being little different from going to mass, attending Catholic schools, and reading Catholic newspapers.

The most interesting chapters of Bakvis's monograph concern the story of the Catholic subculture's sudden collapse. Although the Catholic party still received 85 percent of the Catholic vote in 1963, in 1972 it received only 38 percent. In 1980 the Catholic party merged with the two Calvinist parties to form the so-called Christian Democratic Appeal. According to the author, this sudden and dramatic change was the result of the impact of the many changes that occurred in the Catholic church in general in the 1960s and 1970s. Many of these changes were initiated by Rome and by the Catholic clergy and resulted in considerable loss of hierarchical control. Thus, the Dutch Catholic church, which at one time had been conservative and loyal to Rome, witnessed revolutionary changes—and also enormous losses in numbers and political influence.

Unfortunately, the author does not place these changes in the Dutch Catholic church and the disintegration of the Catholic subculture in the larger context of important social and cultural changes that occurred in the Netherlands in the 1960s and 1970s. What happened to Dutch Catholics appears in this study as an isolated phenomenon, and we are left with the impression that only the Catholic "pillar" of Dutch society came tumbling down in recent years. If Catholics were willing to form a new party with their old Calvinist antagonists something must have happened to the latter as well. But we are not told what happened to non-Catholics.

Also, the research for the most important sections of this monograph is somewhat limited and based on printed materials and a few interviews. Much more needs to be done to give the reader a better picture of a rather complex development that occurred in the Dutch Catholic world in the last two decades. Yet, this is a well-written and most interesting account of an important cultural and political change in Western Europe. Bakvis has given us a good introduction to a complex but fascinating recent event.

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WILLIAM J. STOVER. *Military Politics in Finland: The Development of Governmental Control over the Armed Forces*. Washington: University Press of America. 1981. Pp. viii, 207. Cloth \$19.75, paper \$9.75.

In the introduction to this book, William J. Stover declares that "the scholar must plunge into the

depths of history" in order to understand how the state's political authority controls its armed forces. Unfortunately, Stover does not appear to have equipped himself with reliable diving gear for this task. His survey of Finnish history from Swedish times to the outbreak of the Winter War abounds with elementary errors and is written in a highly simplistic style.

The author appears to be unaware of the growing volume of scholarly works that have helped shed new light on the relationship of Finland and Russia in the nineteenth century and on the political, social, and international aspects of the civil war. Stover relies heavily on out-of-date literature and on memoirs that are not always the most reliable sources of information. He is rarely prepared to adopt a critical attitude toward his sources: Marshal Mannerheim's memoirs are frequently cited, but there is no mention of Stig Jägerskiöld's multivolume biography. The absence from the bibliography of so many relevant works by Finnish authors—such as Helge Seppälä's study of defense policy in independent Finland and Kari Selén's work on Mannerheim and the Defence Council—would suggest that the author has difficulty in reading Finnish. Regrettably, he does not even seem to be aware of works in English. Had Stover read a useful article on the formation of the White and Red forces that appeared in the *Scandinavian Journal of History* (1978), he might have avoided some of the errors and confusion that mar his third chapter. Chapter 4 is little more than a résumé of an excellent but dated work by Marvin Rintala. We learn nothing of Mannerheim's involvement in the plans for intervention against Russia, and the existence of army lists, which might well be used for a detailed analysis of the political attitudes of officers in the armed forces, would seem to be something beyond Stover's ken.

This is an intensely irritating book since it blithely ignores all the careful and patient work of a whole generation of historians who have revealed entirely new dimensions of Finnish history. Statements such as "most Finnish speaking inhabitants remained tied to the land as tenant farmers" (p. 30), seemingly in the Middle Ages (chronological exactitude is not one of Stover's strong points), make one wonder if Viljo Rasila (yes, he has also written in English on the tenant farmers) might not have been better employed in basket weaving. A brief perusal of the Finnish constitution would have told the author that it is the president and not the prime minister who is empowered to call an extraordinary session of parliament. Indeed, an account of the provisions of that constitution might also have thrown some light on what is, after all, the central theme of this book: the relationship between state authority and the armed forces.

It would be tedious to take up more of the errors and dubious assertions made by Stover. One cannot

but marvel at his boldness and confidence in venturing so inadequately equipped into an area where many more seasoned historians have come to grief. In the end, however, one wishes that he had not ventured forth at all.

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PETER BLICKLE. *The Revolution of 1525: The German Peasants' War from a New Perspective*. Translated by THOMAS A. BRADY, JR. and H. C. ERIK MIDELFORT. Baltimore: Johns Hopkins University Press. 1981. Pp. xxvi, 246. \$20.00.

PETER BLICKLE. *Deutsche Untertanen: Ein Widerspruch*. Munich: C. H. Beck. 1981. Pp. 160. DM 28.

Peter Blickle's *Revolution von 1525* was the most important of many studies appearing in the 1970s upon the 450th anniversary of the German peasant war. First published in 1977, with an expanded edition in 1981, it now comes to us in an impeccable and idiomatic English version that also includes a translators' introduction giving a simple outline of the course of the rebellion and its military denouement. Although addressed to the scholar, the book is eminently suited for use in Reformation courses, and it is to be hoped that Johns Hopkins will soon put it out as a paperback.

No one who reads this work will ever again think of 1524–25 in the old way. Indeed, "peasant war" and "rebellion" are likely to disappear from our teaching vocabulary in favor of what Blickle clearly intends as the more honorable—as well as more accurate—designation: "the revolution of the common man." A consensus going back to the Reformation period itself has been misrepresenting the events of the mid-1520s as much less than they actually were. To discredit revolution, they have been associated almost exclusively with the peasantry: primitive rustics living in rural isolation. To trivialize them, they have been ascribed chiefly to a misunderstanding of Luther's religious message: materializing the Gospel and confusing the spiritual and worldly realms. To forestall revolution in the future, it was concluded from the experience of the 1520s that Germans have no tradition and no talent for political planning and organization: when they try revolution they make a mess of it. This is the image of "peasant revolt" and "peasant war" as seen from the top.

The picture is not merely one-sided; it is wrong, and Blickle supplies a vigorously argued corrective resting on massive evidence (for citations to much of this evidence the scholarly reader must go to the German original; space limitations forced their omission from the English version) and relying on his own archival research in South and West Germa-

ny and on the work of agricultural and economic historians such as David Sabean and Wilhelm Abel. Blickle's finely shaded reconstruction of events may be summarized as follows. A crisis in feudalism produced by deteriorating conditions in agriculture on the one hand, and by administrative and fiscal incursions by the state on village communes on the other, led to rural protest in the late fifteenth century and to a crescendo of calls for redress and change. The Twelve Articles, drawn from local grievances, were the representative expression of these demands. Blickle makes a very careful analysis of grievance lists from all over Germany; their remarkable uniformity permits him to advance to general statements about the causes of unrest: serfdom, or—rather—the trend in the half-century or so before 1525 to make it more rigid; humiliating usage restrictions in forest, field, and stream; forced labor and death duties; the state's (most often the petty state's) attempt to turn its people into arbitrarily taxable subjects. The fight against these trends was carried on in peasant communes—well-organized institutions in which rural people exercised self-government—and frequently in territorial assemblies where peasants had gained a voice. When unsuccessful in anchoring their demands in the “ancient law” (because “tradition” often favored the landlords) they turned instead to Biblicism for their justification. Not Luther, but Zwingli, inspired this shift in ideology, for in his Sixty-Seven Conclusions of 1523 the Zurich reformer had declared the Gospel to be normative for a Christian state. From then on the “godly law” and the “Christian Association” became the principles and goals of the revolution that, far from being a denial of evangelical principles, as orthodox Protestantism judged it to be, was, in Blickle's view, “an unfolding of the Reformation.” But the Gospel was not the cause of the outbreak. Economic oppression lay at the root of the peasants' loss of patience and their resolve to turn the situation around.

To these events in the countryside, sympathetic response was made almost at once in the empire's towns and cities, with additional outbreaks occurring in the mining population. Whatever their differences in condition and aims, these groups could unite on the goal of enforcing God's law. The stage was now set for the revolution of the common man against lordship and authority. Common men were all those who could not govern, or whose ability to govern themselves was being taken from them. They were the powerless in city, town, and countryside. Their aims in rising against their condition of servility was to implement the “godly law” and reorganize society on the model of the “Christian Association” and the principles of “common good” and “brotherly love.” There was no shortage of concrete plans, some realistic, some utopian, for

putting these goals into action. Such schemes help Blickle define the revolution as “a deliberate movement, proceeding on a rational course and with challenging ethical claims, for human self-realization.” Nor was the movement entirely without issue in the aftermath of its bloody repression. At least in the south of the empire, governments showed themselves more responsive than before to the interests of ordinary people as the common man gained representation in territorial assemblies. Blickle therefore rejects the view that the “lost revolution of 1525” is causally linked to the emergence of the absolute state. If it *was* a lost revolution, it nonetheless changed the complexion of German politics. On the other hand the Reformation had to be stripped of its power to create social and political disruption. It was taken over by the rulers. Fifteen twenty-five was the great divide in the history of the German Reformation.

Deutsche Untertanen goes over much the same ground but shifts the emphasis from revolution to political opposition, specifically to the confrontation between common men as political subjects and sovereigns bent on realizing their absolutist ambitions. It is a bracingly argued essay displaying the revolution of 1525 in the larger frame of German history and endeavoring to overturn the offensive stereotype of the German as a passive and servile subject, a cringing *Untertan* as exemplified by the hero of Heinrich Mann's novel of that title. To demonstrate that German *Untertanen* played a creative role in events, and to plead for the reinterpretation of German political history in recognition of this fact, Blickle takes a close look at society from below, focusing on the *Gemeinde*, the village and urban commune, with its highly structured and pragmatically effective procedures of local government, and on territorial assemblies in the southern sections of the empire, where subjects were able not merely to voice grievances but also to help translate these into legislation. Readers of Blickle's *Landschaften im alten Reich* (1973) will be familiar with these matters. But the present book is addressed to a broader range of readers to whom it may still be news that the fifteenth and sixteenth centuries saw a vigorous attempt on the part of common men to build a German commonwealth on a communal basis, that this attempt was far from doomed to failure, and that it was, in fact, successful in politicizing people's sense of themselves and of their place in the state.

Not all scholars accept the main thrust of Blickle's interpretation, and he has been criticized for tolerating weak links in the chain of his argument. But there is no doubt that the two books reviewed here propose a liberating view of German history in the early modern period. Freed of the old presuppositions about the “peasant war” and its relations to the

Lutheran establishment, we may now turn our attention from council chamber, consistory, and theological faculty to the lively action below. This shift should enable us at long last to do justice to the positive aspects of a complex and eventful chapter in German history.

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GERHARD SCHORMANN. *Hexenprozesse in Deutschland*. (Kleine Vandenhoeck-Reihe, number 1470.) Göttingen: Vandenhoeck und Ruprecht. 1981. Pp. 140. DM 13.80.

The introduction to this modest and intelligent little essay points out that Germany's professional historians have contributed very little to the international revival of witchcraft studies since 1960, and no one before Gerhard Schormann had even begun to use the largest modern source collection of German witch trials. That collection was assembled by none other than Heinrich Himmler, who created a *Sonderkommando* for the purpose in 1935. For eight years, its staff ransacked archives throughout the Third Reich—thirty-nine territorial archives, sixty-six communal archives, fourteen noble family archives—to create a central file with 3,621 portfolios (3,052 from Germanic lands, the remainder from other *Herrenvölker*) holding about thirty thousand individual entries. After the collapse of Himmler's world, his collection and its working library ultimately moved to Poland; it gathers dust in Poznan, undisturbed by scholars from either East or West Germany.

Schormann, whose 1977 monograph on witch trials in northwestern Germany forms a valuable counterpart to Erik Midelfort's study of southwestern Germany, has used Himmler's collection as a general guide to the chronological and geographical distribution of witch trials within the Holy Roman Empire. This collection is incomplete, useless for social history, and inaccurate in detail; but it is sufficiently thorough to enable Schormann to identify three general peaks of persecutions around 1590, 1630, and 1660 and to map out zones of heavy and light witch trials. The latter include the lower Rhine, the north and east (except for Mecklenburg), and Bavaria. The *Kernzone der Hexenprozesse* stretches from Lorraine northeast through the Archbishopric of Trier and the Duchy of Westphalia to Minden; across to Schaumburg and Anhalt; then south through Ducal Saxony and the Bishoprics of Bamberg, Eichstätt, and Augsburg to the Swiss border; it also includes parts of Silesia in the east, in addition to Mecklenburg (p. 65). Bavaria, long presumed to be a center of witch trials, has

only about five hundred entries in the Poznan collection—not many considering its size.

Schormann's emphasis on smaller states, especially ecclesiastical principalities, as epicenters of German witch-hunts fits well with Midelfort's paradigm from Baden-Württemberg. But Schormann is unwilling to apply or refine Midelfort's confessional paradigm, partly because Germany's largest Calvinist state (the Palatinate) has no surviving records. Nor is he willing to apply Muchembled's arguments about religious acculturation to Germany, although he offers interesting evidence that pressure to start witch-hunts came from beneath more often than from above.

Schormann's book includes such useful details as the true identity of Spee's opponent, Professor Hermann Goehausen (pp. 37–39), and a fine section (pp. 84–89) on the economics of witch-hunting, demonstrating that they rarely enriched territorial lords. He is acutely aware of the underdevelopment of German social history; his final chapter on the basic explanations for witchcraft draws almost exclusively on English, French, and American scholarship. Yet he has created an up-to-date, brief synthesis on the state of witchcraft scholarship concerning Germany that compares extremely well with its totally outmoded counterpart in the "Que sais-je?" series—thanks in part to Heinrich Himmler.

E. WILLIAM MONTER
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BERND MÜTTER. *Die Geschichtswissenschaft in Münster zwischen Aufklärung und Historismus: Unter besonderer Berücksichtigung der historischen Disziplin an der münsterschen Hochschule*. (Geschichtliche Arbeiten zur Westfälischen Landesforschung, Geistesgeschichtliche Gruppe, number 1.) Münster: Aschendorffsche Verlagsbuchhandlung. 1980. Pp. 521. DM 84.

Bernd Mütter's theme is the emancipation of historical thinking in Münster from what served in that city as the functional equivalent of the Enlightenment in Protestant areas of Germany: the normative traditions of Catholicism. Mütter thus seeks to provide an underpinning at the local level for Friedrich Meinecke's Olympian view of the rise of historicism. His story begins with the increasingly conservative circle of historians that formed around the minister and founder of the old university in the late eighteenth century, Franz Freiherr von Fürstenberg. It continues through the liberal neohumanism of Wilhelm Heinrich Grauert between 1835 and 1850 and, after 1850, the conflicting tendencies represented by conservative Catholics and by one Johann Josef Rospatt. Mütter sees Grauert's significance as having been "epochal" for the emergence of autono-

mous historical thought, but he insists nonetheless that his idealization of classical antiquity prevented Grauert from attaining true historicism. Some of the Catholics who followed him, especially Julius Ficker, made the requisite distinction between religious commitment and scholarly objectivity, but they did not remain at Münster very long. Those who did remain succumbed to conservative romanticism. Rospatt, a curiously anomalous figure, retrogressed in another direction, attempting to revive eighteenth-century rationalism. It was only in the last quarter of the century, with the arrival of the Protestant Theodor Lindner (one of Ranke's students) and finally of the Catholic Heinrich Finke, that historical studies in Münster became truly autonomous from prehistoricist inhibitions.

Mütter's volume, originally written as a dissertation at Münster under the direction of Heinz Gollwitzer, one of the university's leading historians in this century, is appropriately placed in the series it inaugurates. It makes an unquestioned contribution to the intellectual history of Westphalia by examining a major strand in the nineteenth-century *Wissenschaftsgeschichte* of its leading academic institution. The author's insistence that second- as well as first-rank thinkers merit the historian's attention is not to be denied, his research is thorough, and his writing is clear and often elegant. Still, this is basically a narrative of the careers of some rather obscure academics. With the exception of Ficker (who taught at Münster for only a year in any case), none were especially influential, except insofar as they helped either to retard or to advance the progress of historicism at Münster, and Mütter does not really attempt to bring out their qualities as representative men of their times. In sum, this is an exhaustively competent study of a narrowly delimited topic.

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JEAN W. SEDLAR. *India in the Mind of Germany: Schelling, Schopenhauer, and Their Times*. Washington: University Press of America. 1982. Pp. ix, 259. Cloth \$21.50, paper \$11.00.

In Jean W. Sedlar's revised dissertation (1970) the three Indian treatises of primary importance are the Upanishads, the Bhagavad-Gita, and the Laws of Manu. Initially, Sedlar offers a succinct evaluation of the place of India in pre-nineteenth-century European culture and then proceeds into an exacting narrative focused on the role of Indian thought in the early German romantic period and in the philosophies of Schelling and Schopenhauer. Investigating such themes as reason and ethics, she concludes that a meaningful Indian influence on Schelling's

work is out of the question. But Schopenhauer showed a compelling interest in, as well as a respect for, Indian ideas.

Analyzing Indian and European thinkers, Sedlar faces a plethora of problems. The philosophies differ widely in place, date of origin, and cultural roots. Schopenhauer's thoughts were internally consistent during his forty-year career. Although maintaining a reasonably uniform underlying outlook, Schelling's ideas underwent notable alterations during his lifetime. The Indian sources themselves were written by unknown authors over a period of several centuries. Sedlar is to be commended for carefully comparing and contrasting the themes developed by these thinkers, especially since the Indian texts do not use philosophical terms in any precise manner. Perhaps a greater sense of unity could have emerged if she had focused more on the questions posed than on the answers proposed. To do this, an explication of the historical cultures involved would be needed, but her study would have gained in value by comparing cultures and the similar philosophical responses that they nurtured.

Sedlar's study shows little sensitivity to the dynamic interaction that exists between a thinker and his culture. Her chapter on morality, for example, commences with a long section on Indian thought, proceeds to Schelling, and culminates with Schopenhauer with virtually no attempt to integrate the three perspectives through a rigorous historical and philosophical analysis. Her ahistorical approach simply does not help the reader understand the issues confronting the German and Indian thinkers under consideration.

Given the complexity of Sedlar's topic, a more fully developed index and a more extensive bibliography would be required to appreciate her struggle with the diverse thinkers of her study. Familiarity with Peter Hanns Reill's *The German Enlightenment and the Rise of Historicism* (1975) would have resulted in her awareness that a historical consciousness was emerging in Germany even before Herder. Comprehending this historicist development could have helped Sedlar understand the interest that Germans had in various civilizations, including India. In the final analysis Sedlar fails to delineate the concrete, historical issues with which her thinkers wrestled as they responded to the quandaries of their respective eras. She fails to convey the excitement of the dynamic and symbiotic relationship existing between man's mundane milieu and his soaring ideas.

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WOLFGANG RENZSCH. *Handwerker und Lohnarbeiter in der frühen Arbeiterbewegung: Zur sozialen Basis von Gewerkschaften und Sozialdemokratie im Reichsgrün-*

dungsjahrzehnt. (Kritische Studien zur Geschichtswissenschaft, number 43.) Göttingen: Vandenhoeck und Ruprecht. 1980. Pp. 260. DM 64.

This dissertation is part of the widespread tendency of combining the history of the labor movement with labor history, the history of organizations and ideas with the history of the working class. It deals with a crucial period of the German labor movement: the initial years of the Bismarckian Reich, that is, the period of the early rise of the labor movement and the worst economic crisis of the nineteenth century and of incisive structural changes of industrial work. Wolfgang Rensch has chosen Berlin as a city with a strong socialist labor movement and a highly differentiated working class. He concentrates on four trades: the booming building industry with a predominance of skilled workers and a strong position of the trade unions; the clothing industry with a depressed situation of workers, sweatshops, and weak trade unions; the textile industry with a high influx of unskilled labor from agrarian regions and little organized labor; the machine-building industry with a wide variation of work, a strong labor aristocracy, many small labor organizations and clubs, but no general trade union.

The study does not match the high standard of recent working-class studies of America and Europe. It gives only general overviews of the place of work and does not look into family life, into the structure of working-class communities, or into housing, health, schooling, and other aspects of working-class life outside the factory and the workshop. The book has, however, two strong points. It tries hard to relate the history of working conditions to the history of protests, strikes, and local labor organizations rather than virtually omitting labor politics as many other recent studies have done. Moreover, it shows the important stimulating as well as impeding role of artisan traditions in a slow and contradictory transition from movements of journeymen rooted in artisan values and mainly oriented to a preservation of the social status of skilled workers toward a modern labor movement with general trade unions and more general economic and political goals.

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KLAUS TENFELDE and HEINRICH VOLKMANN, editors. *Streik: Zur Geschichte des Arbeitskampfes in Deutschland während der Industrialisierung.* (Beck'sche Elementarbücher; Arbeitsbücher, Sozialgeschichte und Soziale Bewegung.) Munich: C. H. Beck. 1981. Pp. 329. DM 29.80.

After an auspicious beginning in the twenties, research about German strikes ceased in the Nazi

period and only now, some fifty years later, is showing signs of life. Thus Klaus Tenfelde and Heinrich Volkmann's series of essays by younger scholars (almost all are under forty) dealing with strikes in imperial Germany is especially welcome, as is the useful statistical appendix.

Six of the essays deal with specific strikes; two are concerned with middle-class reformers and the social problem; one innovative piece analyzes housing and social conflict in the Saar area; and three others address general issues such as the state and strikes, the development of employer organizations, and the general trends in prewar strikes. A final article by Gerald Feldman draws the reader's attention to the unfinished agenda of strike research in the Weimar Republic, which includes a paucity of studies of strikes in small- and medium-sized firms as well as the political nature of many strikes.

The general tenor of the specific studies is that German employers were unusually harsh and repressive in responding to strikes. Blacklists, lockouts, identity cards, and company unions were all readily resorted to in order to avoid concessions to militant workers. Hans-Peter Ullmann stresses that employers, when the state ceased to give unstinting support to their side, organized their own self-help associations and turned to lockouts and strike insurance to avoid collective bargaining. Even when the emperor and public opinion sympathized with the strikers, as in the Hamburg dock strike of 1896–97, the employers proved to be recalcitrant. Several of the authors express skepticism of Hartmut Kaelble and Heinrich Volkmann's theory that a progressive rationalization of social conflict was manifest in prewar Germany. Ilse Costas finds Siemens using every weapon in its arsenal to break the 1905 strike and the union of electrical workers. Only 16 percent of German workers were covered by negotiated wage agreements in 1913, and 42 percent of strikes ended in complete defeat for the workers. Klaus Saul argues that attempts by the state to forestall social conflict in the private sector never applied to its own employees in the railroads, post office, and mines. It is also clear that the newer industries of chemicals and electricity were every bit as devoted to union busting as the coal barons and iron and steel magnates were. Despite repeated defeats, the free trade unions grew rapidly before 1914, and their leaders, unlike the contributors to this volume, remained optimistic about the effectiveness of strikes.

My only criticism of this excellent volume is that no comparisons are made with labor relations in other major industrial nations; thus Tilly and Shorter's work on French strikes is not discussed—not even their contention that German strikes fit neatly into the European pattern regarding frequency, duration, and level of participation. Now that the spadework has been done in Germany, the

comparative dimension is deserving of more attention.

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JOHN E. GROH. *Nineteenth-Century German Protestantism: The Church as Social Model*. Washington: University Press of America. 1982. Pp. xxii, 614. Cloth \$30.75, paper \$20.75.

John E. Groh's recent volume is a study of nineteenth-century German Protestantism organized around a theme that the author calls the "unwritten charter," namely, the consensus between state and church that the latter would provide a model for the conduct of the larger society, one exemplifying the principle of freedom within authority. Groh explicitly recognizes his indebtedness to Leonard Krieger, who alerted us to the special qualities of the German concept of freedom. He also acknowledges the impact of a personal experience of academic termination for deviant theological views exacted by the Missouri Synod of the Lutheran Church as a compelling source for the theme of this work. Yet it is not an angry book. On the contrary it is fair-minded and inclusive. For historians who find the intricacies of Protestant theology and social policy more accessible in English than in German, Groh's work will be a welcome complement to the classic treatment of German Protestantism by Franz Schnabel in the fourth volume of his *Deutsche Geschichte im neunzehnten Jahrhundert*. Groh additionally makes use of recent research and some valuable statistical information; he gives greater attention to various popular movements while extending the narrative to 1918.

One consequence of the reform in Germany that accompanied the overthrow of the Napoleonic system was the implementation of the Protestant Union in virtually every state: the unification of the Lutheran and Reformed confessions. Originally an expression of religious commitment, the Union increasingly antagonized devout Christians; the German Awakening in many states contained a strong element both of confessionalism and of protest. In one of his most insightful arguments, Groh suggests that the Awakening tended to reassert the spirit of particularism and fostered a kind of surrogate patriotism that resisted liberal demands for German political unification. Presumably this development also reinforced the various elements of bureaucratic exclusivity that precluded the creation of a national Protestant church embracing the several state churches later in the century. The perpetuation of the territorial churches is one explanation Groh offers for the inadequacy of the Protestant

response to the social question. As rural laborers migrated en masse to the cities, they frequently left the jurisdiction of their native territorial church, and no routine process located them in a new urban parish. The absence of a national church, however, did not protect German Protestantism from an intense commitment to political nationalism at the expense of social engagement; Groh describes the increasing involvement of church leaders and their press in the defense of industrial capitalism and their concomitant denigration of social democracy. Johann Hinrich Wichern emerges from this discussion as a sincere and vigorous but misguided figure; his inner mission enjoyed a supraterritorial structure, but Wichern's belief that moral issues rather than issues of class conflict lay at the root of social oppression severely restricted the use to which the mechanism of inner mission could be directed. By the beginning of the twentieth century, the Protestantism Groh presents was so shattered by ideological and social fragmentation that antisemitism and patriotism were among the few sources of unity. Groh's is an ambitious book, one in which the argument occasionally gets lost in the wealth of documentation and one that merits a more incisive conclusion, but the author deserves commendation for setting his sights beyond the tidier proportions of a monograph.

It should be added that the author does not seem to have been well served by his publisher. The book is printed by computer, and probably for this reason, in the interest of appearance, footnoting has been severely reduced. This is a real shame, since one of Groh's accomplishments is the introduction of innovative sources—in some cases superb anecdotes—and the documentation is not always evident from the chapter end-notes. There are too many typos and an inconsistency in the use of German and translated titles—flaws that a good copy editor would have caught. I mention these minutiae because the University Press of America is currently conducting a promotion campaign. This is a time when academics welcome the possibility of economical and rapid publication, but not at the expense of significant scholarly apparatus.

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HANS-JÜRGEN BRANDT. *Eine katholische Universität in Deutschland? Das Ringen der Katholiken in Deutschland um eine Universitätsbildung im 19. Jahrhundert*. (Bonner Beiträge zur Kirchengeschichte, number 12.) Cologne: Böhlau. 1981. Pp. xliii, 544. DM 168.

German Catholics in the nineteenth century were well aware that they were seriously underrepresented among the students and professors at German

universities. Hans-Jürgen Brandt's study, *Eine katholische Universität in Deutschland?* originally a *Habilitationsschrift* in Catholic theology at the Ruhr-University at Bochum, describes efforts to remedy this situation by establishing an independent Catholic university. Using material from the Vatican archives and episcopal archives in West Germany and Austria, Brandt traces these ultimately unsuccessful efforts from the era of the secularization of ecclesiastical territories under Napoleon through the major discussions about a Catholic university in the 1860s.

The publication of *Eine katholische Universität?* in a series on church history is appropriate, because Brandt is much more concerned with the debate within the church over such a university than he is with the fate of Catholics and Catholicism in the existing German universities. Given this limitation, Brandt provides a clear and thorough examination of the many proposals about the possible structure, location, and relation to the church of a Catholic university. In general, both laymen and clerics appear to have based their attitudes on previously adopted views about the relationship of the German church to the state, to Rome, and to modern science and scholarship. The supporters of a separate Catholic university were divided into two main factions: an ultramontane group around Bishop Wilhelm von Ketteler of Mainz, who wanted a church-controlled university similar to that founded at Louvain in Belgium in the 1830s, one that would adhere strictly to the "neoscholasticism" of Pius IX; and men such as Professor Franz Joseph von Buss of the University of Freiburg, who envisioned a Catholic university independent of both church and state. Among the critics were a number of bishops who feared that creation of a single Catholic university of either type would further weaken the influence of religion at other universities. The critics also included professors such as Ignaz von Döllinger in Munich and Johann von Kuhn in Tübingen, who stressed the need to develop Catholic scholarship on the basis of the freedom of inquiry and teaching granted by the traditional universities. Brandt concludes that these divisions within German Catholicism caused the movement for a Catholic university to fail in the 1860s, before any serious effort was made to challenge the anticipated hostility of the state governments to such an independent institution.

Aside from its rather narrow approach, the major weakness of Brandt's book is its excessive length: 350 pages of closely printed text followed by 171 pages of documents. The most interesting section of the text and the major portion of the documents deal with the discussions during the 1860s; but Brandt devotes almost two-thirds of the text to what is, in essence, background material. Students of German church history may want to read the earlier

sections in detail, but nonspecialists may choose to rely on the excellent summary at the end of the book for a distillation of this material on the period before 1860.

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ELLEN LOVELL EVANS. *The German Center Party, 1870–1933: A Study in Political Catholicism*. Carbondale: Southern Illinois University Press. 1981. Pp. xi, 433.

The Center party is the great enigma of modern German history. Jumping with apparent wantonness between left and right, it was also the party of the Catholic minority, which nevertheless enjoyed a pivotal role in the politics of both Wilhelmine and Weimar Germany. Recent years have seen a growing amount of work in English on the pre-1914 party but much less on the Center's subsequent years. Ellen Lovell Evans has now provided a general survey of the party throughout its existence, with a brief look at its pre-1870 origins and a still briefer epilogue on the founding of the CDU/CSU in 1945.

Half the book, and its real focus, is on the Weimar years. The author shows how the prorepublican stance of Erzberger and Wirth was first smothered by the opportunism of those like Wilhelm Marx, then steadily rejected as the party drifted to the right. Valuable points are made about party leaders' fears of a loss of support among educated, middle-class Catholics and the clergy, about the movement of Catholic workers to the parties of the left, and about the paralyzing effect generally of economic interest groups. All of this left religious and educational issues as an even stronger binding element, symbolized by the emergence of the desiccated and ineffectual Prelate Kaas in 1928 as the first cleric to head the party. On some of these and other issues the author perhaps overdraws the extent to which the problems of the party in the Weimar Republic constituted a new departure. But she does emphasize one seemingly paradoxical continuity: that the Catholic integralist and interdenominational currents within the Center were by no means synonymous with a right-left division. The emphasis on Catholic identity actually allowed a greater flexibility in cooperating with the left, whereas interdenominational aspirations meant in practice cooperation with Protestant conservatism. Evans notes the implications of this from the pre-1914 debate over the nature of the party, through the arguments about the Essen program of 1920, to the Christian-Conservative union of 1945.

This is a balanced and unhagiographical book, even if it is perhaps more indulgent to Brüning than many recent historians have been. The play of political interests within the party is clearly outlined,

although the broader political context is sometimes rather hazy: this is especially true of the years 1909–14 and 1930–33. (It is notable that the book, which is not based on original sources, is generally up-to-date in its use of secondary works on the Center itself but does not use some of the most important literature of the last fifteen years on imperial and Weimar politics more generally.) At worst this account drifts into a somewhat relentless recital, above all where complex patterns of regional politics are briefly surveyed. The price that is paid here for inclusiveness is blandness and lack of bite. It is a corresponding virtue that the book covers the Center party over a period covered by no other account in English and does so fairly, accurately, and often thoughtfully. It will prove a useful work of reference for those who do not read German.

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RENATE PORE. *A Conflict of Interest: Women in German Social Democracy, 1919–1933*. (Contributions in Women's Studies, number 26.) Westport, Conn.: Greenwood Press. 1981. Pp. xviii, 129. \$22.50.

In 1919, as Renate Pore shows in this useful new study, the Social Democrats, having secured the vote for women, fully expected to be rewarded with their electoral support. Just to make sure, they launched a major campaign to get the female vote, issuing twelve special pamphlets, publishing a weekly women's magazine, and ensuring that their own women's organization, weakened by the loss of its left-wing leadership in 1917, received adequate funding and support. The result was a grievous disappointment to the party. In this, as in other elections of the Weimar Republic, women tended to vote more for the Catholic Center party than for the SPD. As a result the women's section of the party, charged with delivering the female vote, was discredited, and the party organization lost interest in women's issues. Already in 1918–19, however, an SPD government had presided over the mass dismissal of women workers to make way for returning troops, despite the fact that Social Democratic theory held that work was the key to women's emancipation. In practice, the SPD adhered to a modified version of the bourgeois ideal of the family. Women's primary role was seen as motherhood and marriage. Within the party, as Pore notes, women were excluded from decision making on all major issues of general significance, but they were allowed some authority on issues defined as their own, and indeed the party's political stance on issues such as abortion, divorce, the legal rights of married women, illegitimacy, and prostitution was a remarkably progres-

sive one. Thus the Social Democrats perpetuated, even developed further, the attitudes toward women that they had formed in the *Kaiserreich*.

Pore's brief study is a welcome addition to the surprisingly sparse literature on the SPD women's organization in the Weimar period. It is particularly good on the primacy of electoral politics in SPD thinking. But in general it is too brief to do more than whet the appetite for the full-scale study that has yet to be undertaken. The first forty pages deal with the period before 1918, so we are left with only seventy or so on the Weimar Republic itself. This is scarcely longer than the section of Werner Thönnessen's book on the SPD women's movement devoted to this subject, a study that appeared in English in 1973 and now stands in serious need of revision. Pore clearly disagrees with Thönnessen, but she does not really get down to criticizing his views in detail. Nor is her account of the movement before 1918 very accurate. She has also failed to make use of recent scholarly studies by Stefan Bajohr, Silvia Kontos, Atina Grossmann, Brian Peterson, and Gisela Losseff-Tillmans on working-class women and socialism in the 1920s. Moreover, Pore has based her study only on printed material. Here the major sources used are the SPD's women's magazines and the minutes of the SPD women's conferences—the same as used by Thönnessen. The information they provide mainly covers general questions of policy and ideology. Even here the book conveys no sense of the changes that took place over the fourteen years of the republic's existence and does not tell us in any detail either how the SPD women operated in national and regional parliaments or how they faced up to the problems of the inflation, the depression, and the rise of National Socialism.

Major questions about the social composition of the SPD women's organization, the nature and limitations of its appeal to working-class women, its role in the daily life of activists, and its place in working-class life in a more general sense are unanswered and will remain so until much more is known about the SPD women's movement at a local level. It is a pity that Pore did not make use of the rich material available in local archives and local newspapers or attempt to interview former activists in the movement. Fortunately recent work in the Federal Republic, notably that of Karin Hagemann on Hamburg, has shown that this task is now well in hand, and one awaits with interest the results.

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NIGEL HAMILTON. *The Brothers Mann: The Lives of Heinrich and Thomas Mann, 1871–1950 and 1875–1955*. New Haven: Yale University Press. 1979. Pp. 422. Cloth \$30.00, paper \$9.95.

Nigel Hamilton's *The Brothers Mann* is a deftly woven double biography that initially arouses skepticism but ultimately commands respect. At first the work rather tediously tells of the two most famous twentieth-century German writers' bourgeois childhood and youth in the northern town of Lübeck. Isolated facts about Thomas and Heinrich Mann's family life are described in an anecdotal and disjointed manner. But gradually and almost imperceptibly, the reader is drawn into an absorbing and delicately nuanced account of the animosities and disagreements that eventually led the two brothers to their famous public rift during the First World War. By judiciously quoting excerpts from the brothers' correspondence to each other, selections from their essays and fiction, and relevant passages of letters to others, Hamilton has admirably constructed an engaging drama of sibling rivalry followed by postwar fraternal reconciliation.

In 1904 Thomas wrote: "I have absolutely no interest in . . . political freedom . . . What is freedom anyway? Perhaps because so much blood has been spilled for the word it has something strangely unfree for me" (p. 87). During the prewar years, Thomas was seen by the younger generation of writers as a literary aesthete as well as politically indifferent. He had married into a wealthy, assimilated Jewish family in Munich and outwardly appeared the model of respectability and bourgeois self-satisfaction. His fiction, though, explored the realms of decay and disintegration (*Buddenbrooks*, *Death in Venice*). Heinrich meanwhile consciously rejected the political servility, the increasing nationalism and materialism of Wilhelmine Germany. "What makes me an exception in today's Germany," wrote Heinrich in 1907, "is my radicalism; I am a Radical in spirit, in soul and form" (p. 127). Publication of his most famous works, *Der Untertan* and *Professor Unrat*, bitterly satirized the social pretensions and illiberality of Germany during a time of growing patriotism.

The opening volleys of World War I profoundly estranged the two brothers. Heinrich immediately rejected the frenzy of the August days. In contrast, Thomas seized on the war as the greatest of historical experiences. Exalted by the communal nationalism, he felt compelled to describe its beneficial effects for Germany. In a series of wartime essays, Thomas depicted the difference between the shallowness of Western "civilization," with its democracy and rationality, to the deeply felt German "Kultur," with its inwardness, creativity, and conservatism. Although his arguments advancing Kultur were obviously derived from antidemocratic writers and philosophers such as Lagarde and Nietzsche, his critique of civilization actually was directed closer to home, namely to his own brother's internationalism and liberalism. Heinrich's 1915 essay on the French writer Emile Zola responded to Thomas's politics by

dismissing his devotion to inwardness as a product of "hopelessness." Thomas ultimately replied with his infamous *Reflections of a Non-Political Man* (1918), an embarrassing panegyric published at a time when many wished to see a speedy and just end to the war. At the height of the brothers' confrontation in 1918, Thomas wrote vitriolically to Heinrich: "Why was it [the Zola article] in its savage polemic aimed at me? Fraternal experience drove you to it. . . . Let the tragedy of our brotherhood take its course to the bitter end" (p. 183).

During the Weimar Republic the brothers reconciled, and Thomas gradually came to share many of Heinrich's democratic views. Thomas achieved his greatest recognition with the publication of *Magic Mountain* and the receipt of the Nobel Prize in 1929. Heinrich became president of the literary section of the Prussian Academy of Fine Arts in 1931. But the Nazis' coming to power in 1933 forced both into permanent exile in France and America. Heinrich died in virtual obscurity in 1950. Thomas moved to Switzerland during the McCarthy era and died there in 1955. Unlike Heinrich, his fame continued unabated throughout the postwar years.

The author's decision to emphasize the brothers' relationship to one another excludes much that one would normally consider vital to a biography. For example, little is told of other associations with wives, children, or professional colleagues. This excision results in a somewhat one-dimensional portrayal; the Manns were seemingly obsessed both in life and in art with dissecting and analyzing their familial and political relationship to each other. Yet Hamilton's approach does work well. It allows one to realize that the fraternal drama of Thomas and Heinrich mirrored the larger drama of the twentieth-century German intellectuals' connection between their artistic work and their social engagement. The biography is a work that can be highly recommended to those who wish to understand the intellectual and political dilemmas faced by two highly creative writers during the most unsettling of times.

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HOLGER G. HASENKAMP. *Die Freie Hansestadt Bremen und das Reich, 1928–1933: Eine verfassungsgeschichtliche Untersuchung.* (Veröffentlichungen aus dem Staatsarchiv der Freien Hansestadt Bremen, number 47.) Bremen: Selbstverlag des Staatsarchivs der Freien Hansestadt Bremen. 1981. Pp. 301.

The vital significance of the regional and local dimensions of the Nazi takeover of power has long been recognized and has prompted a number of

studies of individual *Länder*. Holger G. Hasenkamp's book is a dissertation prepared within the Faculty of Law at Freiburg University and has both the merits and some of the drawbacks of a primarily legally oriented approach to history. On the positive side, the author provides a very solid account of the effects on Bremen of Reich legislation. He shows the extent to which presidential decrees during 1930–33 helped to erode the already limited independence of Bremen vis-à-vis the Reich, particularly in relation to police and budgetary powers, thereby helping to prepare the “political-psychological climate” for the Nazi *Gleichschaltung*. He also has one or two interesting points to make about Nazi legislation. Thus he insists on the extent to which the Reichstag Fire Decree, far from being a peculiarly unscrupulous and uniquely Nazi invention that was either prepared long beforehand or in a totally ad hoc fashion, was in fact largely based on previous emergency decrees of the Weimar period. This confirms a point made by Hans Mommsen in his controversial article on the Reichstag fire. Finally, Hasenkamp suggests that the drafting of the first *Gleichschaltung* Law of March 31, 1933, was influenced by the experience gained from the problems encountered in the takeover of power in Bremen. If this is true, it reinforces the view of the Nazi takeover as a process in which, to a considerable extent, the Nazis were responding to the development of events as well as shaping them.

The negative side of the legalistic approach emerges in the author's eagerness to define to what extent the actions carried out by Papen or the Nazis were legal within the terms of the constitutional arrangements of the time. Hasenkamp criticizes Bracher for applying criteria outside these terms of reference in his judgments on, for example, the Reichstag Fire Decree. He himself considers the decree to have been constitutional as such but illegal when applied for the takeover of power in Bremen. This is not, however, an issue of much interest to historians. Hasenkamp provides a solid account of the takeover of power in Bremen, benefiting from access to Reich Ministry of the Interior files in Potsdam. But it is largely limited to the governmental sphere and so contains few surprises. He does, however, bring out the extent to which Bremen pursued a hard line on such issues as the application of the Aryan clause to civil servants or the rounding up of communists in a desperate attempt to live down its liberal past. And he shows how Bremen businessmen energetically proclaimed their loyalty to the new regime in the hope of not losing out in the competition for its favors to their neighbors and rivals in Hamburg, another example of the mechanism of *Selbstgleichschaltung*. More work needs to be done on the impact of the seizure of power on the various economic and social groups at regional and local levels. But it would be a little unfair to expect

this of a legal dissertation. This one performs its limited task well.

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REINHARD NEEBE. *Grossindustrie, Staat und NSDAP, 1930–1933: Paul Silverberg und der Reichsverband der Deutschen Industrie in der Krise der Weimarer Republik*. (Kritische Studien zur Geschichtswissenschaft, number 45.) Göttingen: Vandenhoeck und Ruprecht. 1981. Pp. 314. DM 64.

The last several years have witnessed intensive study of the connections between German big businessmen (or German capitalism), the undermining and collapse of the Weimar Republic, and the transfer of power by the political elite to the mass-based Nazi party. Unlike many earlier studies, recent work by Hans Mommsen, Dirk Stegmann, H. A. Turner, Bernd Weisbrod, and others has enjoyed the benefit of substantial access to the archives of powerful industrial organizations and personages. Reinhard Neebe's analysis of industrial politics, and especially of the role of sometimes-initiator, sometimes-bellwether Paul Silverberg, is fully anchored in these sources and furthers our knowledge of what transpired at numerous crucial moments. Although the author tells us much, however, he explains less, and his aversion to both conceptualization and aggregation allows the reader to put Neebe's findings in the service of any number of interpretations, some of them more compelling than any implied by the few conclusions he allows himself to draw.

This is a shame, because Neebe's own narrative is constantly suggestive and touches on the key issues; his self-restraint in dealing with them appears intentional—he wants to make up for “an empirical deficit” (p. 18)—but it is not fruitful: attempting to relate politics and economics while eschewing all theoretical reflection is a bit like fishing with a hoop in place of a fishnet. This deficit, for example, is rather striking in Neebe's otherwise very rich discussion of the divergence between, on the one hand, those industrialists who sought first to cooperate with the SPD and then, during the economic crisis, to weaken it by compelling it to participate in the rollback of labor's gains, and, on the other hand, those who simply supported an authoritarian restoration that would end mass politics altogether. Neebe supplies the reader with a number of suggestive hints about what might have been important categories (“Western industry,” “heavy industry,” “leadership levels,” “Reusch group,” “moderate heavy industry,” “modern growth industries,” “Hitler wing of industry,” and so forth), but the reader gains little sense of what motive forces operated beneath the surface of events, particularly since the SPD and unions are themselves absent from the narrative. Part of the problem lies also in the

author's particular version of the "primacy of politics" and aversion to Marxist analysis, which he unfortunately equates with the formulas of the Third International and several East Germans. It is all well and good to hold premodern figures and attitudes responsible for the undermining of the republic and the appointment of Hitler as chancellor—Neebe is certainly right to treat Brüning and Papen very harshly—but, particularly in a book about the politics of German industrialists, it is a bit of a cop-out to conclude that their campaigns against democracy and parliamentarism reflected premodernism. There was some causal connection between industrialists' "premodern" attitudes and their very modern interests, and the Nazis may have disappointed them in one regard but not in the other.

If Neebe is not fully successful in explaining the relationships among big industry, state, and the Nazis, he is very illuminating on the subject of Paul Silverberg, one of Weimar's most organizationally and politically active industrialists. Silverberg has tended to enjoy a good historical press: the captain of industry who held out an olive branch to labor in 1926, who supported industrialist-union cooperation, who opposed the transfer of power to Hitler while attempting to consolidate a popular alternative, and who at least left Germany after the Nazis came to power. Neebe convincingly demonstrates that Silverberg, a Jewish-born lignite magnate whose original aspiration was to become a professor, actually sought to cooperate with the unions in order to split them off from the SPD and weaken the democratic state, entertained brutally reactionary social conceptions, and in fact campaigned for a Hitler chancellorship on the grounds that the Nazis could and would deliver massive popular support for the private economy. Worst of all, even after he had been stripped of his offices for racial reasons and had moved to Switzerland, he continued to support the regime in and of his fatherland, along with its racist and imperialist policies. Neebe even conveys the impression that, had Silverberg personally been spared, he would have joined Krupp and his other genteel colleagues in their cowardly abandonment of Jewish colleagues of all political persuasions. It is indeed astonishing and embittering that a group that could show so much "courage" and resilience in its fight against social democracy could prove so utterly craven and morally bankrupt in the face of Nazi reaction.

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KONNILYN G. FEIG. *Hitler's Death Camps: The Sanity of Madness*. New York: Holmes and Meier. 1981. Pp. xxiv, 547. Cloth \$39.50, paper \$19.50.

This is an important, extremely useful, badly needed book—the kind of work whose liabilities are turned into assets by the nature of the subject and the emotional energies expended by the author in dealing with it. It is the first such work in the growing literature on the Holocaust to attempt, and largely achieve, several important objectives. Konnilyn G. Feig has developed a comprehensive profile of all the major installations in the system of Hitlerian concentration camps and extermination complexes. She has demonstrated how critically the permanent killing centers in the wartime German-occupied east depended upon the prewar experience of the permanent concentration camps within the Reich. She has shown, in a word, much about the process by which the latter grew into the former. If the "twisted road to Auschwitz" was as short and straight as some of us have come to believe, that reduction in distance was primarily due to the preparations for destruction that had been developed and accomplished in the prewar camp system.

The history of each camp as an SS instrument is accompanied by an extensive and specific analysis of how the individual facilities inflicted different sufferings on the victims who were ground through its machinery of destruction. The hell of the camps was a universally hideous and horrible phenomenon. That hell, as Feig graphically illustrates, had many different forms and peculiarities, individually conditioned by location, the SS personnel, and any economic or military functions ancillary to the principal enterprise of liquidation. As a result, each camp bludgeoned its victims into a society of survival with different characteristics. This is probably the strongest and most commendable feature of Feig's work—the unsparing compilation of the collective camp experience of the victims in a synthesis written with sensitivity, power, and emotion.

The supporting documentation, the appendixes, and the bibliography generally are quite good. There is little that is new here, but the overwhelming mass of what has been discovered about the camps has been compiled and is here. For the beginning student or general reader searching for an introduction to the Holocaust, or for the specialist interested in focusing on the collective phenomenon of the concentration camps, this book is as good a place to begin as we now have.

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ANGELA STENT. *From Embargo to Ostpolitik: The Political Economy of West German-Soviet Relations, 1955–1980*. (Soviet and East European Studies.) New York: Cambridge University Press. 1981. Pp. xvi, 328. \$39.50.

Ever since the establishment of the Soviet Union the Western nations have sought to influence Soviet foreign policy through their trade policies. The results of these attempts have generally been dismal. The Soviet Union, despite its varied economic problems, has proven surprisingly unmalleable to economic pressure from the West. The historical experience of the interaction of politics and trade in relations between the Soviet Union and the Federal Republic of Germany in recent years provides rather striking evidence explaining why the West has had so little success in this linkage problem. In the first detailed attempt in English to examine the development of Soviet-German trade relations, Angela Stent discovers that the West German government's efforts to alter Soviet policy on such key issues as the German question and the status of West Berlin came to naught because they touched on the "core values" of the USSR, core values being vital interests of a nation, like its security, prestige, and territorial integrity. Stent argues that, although both West Germany and the USSR tried to use leverage in their economic and political relations, this linkage was rarely successful. In fact, linkage was usually successful only when secondary values, that is, values not involved with national security or prestige but with increasing international influence or satisfying domestic political groups, were at stake.

In tracing the changing dynamics of Soviet-German relations since the founding of the Federal Republic, Stent finds that most of her evidence suggests that, when the Germans followed a negative political linkage strategy based largely on the influence and control by the U.S., little could be done with the USSR. In the period 1949 to the mid-1960s, when the Soviet trade was highly politicized, the Soviet Union stubbornly refused concessions in the political arena. The author cites in particular the case of the pipe embargo of 1962, which resulted in more of a headache for U.S.-German relations than it was for German-Soviet relations. Since that highly controversial case the Germans have reduced the politicization of their Soviet trade policies and achieved much more from the Soviets. The Brandt Ostpolitik strategy in particular has reduced some of the parameters of politicization between the two nations and improved the political environment, allowing for more maneuvering between both nations. As a result both nations' economic and political goals and values have become more symmetrical and chances for improving relations have become better.

The volume of trade between the USSR and West Germany has never been particularly great, amounting to at most 7.6 percent of the Soviet foreign trade in 1975, but this belies its importance in terms of usefulness to the Soviets. The Soviets obviously need and want further German trade in

the areas of high technology, but only at their price. As Stent concludes, based on the West German experience, "it is in general illusory to believe that the West can significantly change Soviet political behavior through the use of economic levers" (p. 19).

This is a solid, balanced study of the complexities of the attempts to link trade with politics, and as a political scientist Stent has a fine grasp of the historical problems involved. She also has a cool, detached view of the possibilities of using trade sanctions against the USSR. Her conclusions are worth pondering by scholars as well as diplomats in the West.

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HEIDI BORNER. *Zwischen Sonderbund und Kulturkampf: Zur Lage der Besiegten im Bundesstaat von 1848*. (Luzerner Historische Veröffentlichungen, number 11.) Lucerne: Rex. 1981. Pp. 272. 36 FR.

The purpose of this book is to show the general state of Swiss conservatism between 1847 and 1874—a crucial period in Swiss history. The constitution of 1848 and the revisions of 1874 established a modern, secular state with a stronger central government. This was entirely the work of liberals. When the Sonderbund of the conservative cantons collapsed in 1847, Swiss conservatives became the perpetual minority. On the federal level major issues were decided by debate within the party of the liberals. And as the Kulturkampf against a reassertive papacy intensified, conservatism became a more distinctly confessional party (eventually the voice of Switzerland's "Catholic ghetto"). These events are well known, for Swiss party history is a well-researched field.

Heidi Borner's approach is to look at the period through the eyes of leading conservative statesmen. Using chiefly newspapers and private papers (especially those of Lucerne's Philipp Anton von Segesser), she shows us the mind and feelings of those who decried the loss of local autonomy and therewith all that had given Switzerland its character. For the general historian this book affords a new perspective on the history of the two original "isms." The possibilities and nature of many European developments often become clear in the peculiar form they take in Switzerland.

HARRY F. YOUNG
Department of State

RONALD F. E. WEISSMAN. *Ritual Brotherhood in Renaissance Florence*. New York: Academic Press. 1982. Pp. xiii, 254. \$27.50.

Ronald F. E. Weissman begins his study of Florentine confraternities from 1250 to the Catholic Reformation with a picture of the city partitioned into family and especially neighborhood groups: the "neighborhoods and quarters resembled cities in miniature, each with its own local services, resources and solidarities" (p. 9), and the neighborhoods segmented Florence "into multiple communities" (p. 10). Social contacts in these urban villages were "agonistic" and intimate, creating a divisiveness ameliorated only by "chains of patronage and friendship" (p. 24).

During the fourteenth and fifteenth centuries, the confraternities created such patronage chains by recruiting members from different neighborhoods. Weissman argues that through their election procedures and the rotation of short-term offices, the confraternities were miniature communes, providing, in effect, an apprenticeship in civic responsibility for a broad spectrum of men otherwise loyal only to kin and neighbor. His thesis works most satisfactorily for the *disciplinati* companies, which specialized in collective flagellation rites and which appealed to unmarried patricians and tradesmen from all over the city, who characteristically joined during their twenties at a stage in their lives when they were breaking away from their families and neighborhoods in hopes of joining the civic elite. The *laudesi* companies also had members from throughout the city, but the preponderant number came from the same quarter. These were older, married or widowed shopkeepers and petty tradesmen who as lay trustees of an important shrine expanded their patron-client relationships beyond the neighborhood to the quarter but not as far as the entire city.

During the fifteenth century craft and parish confraternities also appeared, a fact Richard C. Trexler cites as evidence for his "ritual revolution" under Lorenzo de' Medici and Savonarola. Weissman argues, in contrast, that these new types were insignificant until the last half of the sixteenth century. In response to both the Medici grand dukes' attempts to transform republican Florence into a court society and the Catholic Reformation concern for a controlled sacramental piety, these guild and parochial sodalities weakened and supplanted the traditional, cross-neighborhood companies.

Weissman's thesis that the neighborhoods and parishes fragmented the social order deserves a full hearing and further testing. Since pre-Tridentine parish priests kept poor records, it has often been assumed that parish, and hence neighborhood, life was weak before the Catholic Reformation, but Weissman's book contributes to a growing body of evidence that indicates otherwise. Weissman's reliance on the anthropology of the Mediterranean village as his guide to the structure of the Florentine

neighborhood-parish is more doubtful, particularly since Florentines saw an enormous gulf between themselves and the *villani* and since the borders of neighborhoods were decidedly vague when compared to the confines of a village or walled town. One might also ask for a more precise explanation of how the sociology of patronage—a hierarchic concept if there ever was one—can illuminate the egalitarian, liminal activities of the religious brotherhoods. His application, however, of well-planned statistical methods to the archival sources asks the right questions and avoids the redundant accumulation of narrative details. Weissman's eclecticism has helped him write a thoughtful, provocative, and important book, which should be the touchstone of all future studies of confraternal life in late medieval and early modern Europe.

EDWARD MUIR
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DAVID B. RUDERMAN. *The World of a Renaissance Jew: The Life and Thought of Abraham ben Mordecai Farissol*. (Monographs of the Hebrew Union College, number 6.) Cincinnati: Hebrew Union College Press; distributed by KTAV, New York. 1981. Pp. xvi, 265. \$20.00.

An important historiographical trend of the 1980s is the integration of Jewish and Christian sources in one mainstream historiography. *The World of a Renaissance Jew* is a model of what can be accomplished by a historian conversant with Hebrew and Latin texts and knowledgeable in Jewish history and Renaissance history. Through archival investigation of the Hebrew manuscripts that Abraham Farissol (1452–ca. 1528) copied for patrons, David B. Ruderman has followed Farissol from his boyhood in the cosmopolitan Jewish community of Avignon through his adulthood as resident in Ferrara and as itinerant man of letters within the Italian city-states. Part 1 is the first full-scale reconstruction of the biography; part 2 is a contextual analysis of Farissol's confrontation with Christianity; and part 3 is a presentation of Farissol's conservative stance vis-à-vis philosophy and his open-minded, yet messianic, geography.

While Ruderman accepts Cecil Roth's view that the Italian Renaissance was an age of relative toleration, his emphasis is on relative toleration compared to inquisitorial Spain or Counter Reformation Italy. Thus François Tissard, author of a Greek and Hebrew grammar in 1508, wrote of his great appreciation and respect of his Hebrew tutor—probably Farissol—despite the Frenchman's utter scorn for the Jewish religious services he beheld in Ferrara and his essential position, "Be converted!" (p. 102). Ruderman contributes to the revisionist

position recently expounded in Hebrew by historian R. Bonfil with an exposé of the conversionary tactics of Christian Kabbalists and of Ficino and Pico della Mirandola in particular. Ruderman revives Joseph Perles's thesis, based on a letter of Ficino, that Abraham Farissol, along with Elijah Delmedigo, debated the converted Jew Mithridates at the home of Pico della Mirandola in 1485; likewise he builds a case that Farissol was a disputant at the court of Ercole I d'Este in 1487. Farissol's polemical manuscript of 1512, "Magen Avraham," communicated to the Jewish community the dangers inherent in religious syncretists whose spiritualized interpretation of Judaism and Christianity served the old purpose of drawing Jews to Christianity.

Farissol's re-evaluation of usury in terms of Thomist just price theory was a well-reasoned response to the dangers befalling the Norsa and other Jewish banking families in the wake of Franciscan preaching. Farissol's traditional upholding of his faith in response to the philosophies of Pomponazzi and Gersonides paralleled the Christian defenses of Giles of Viterbo. Farissol's numerous observations on the natural world were a cultural trait that he absorbed from Farissols in Avignon who were active in astronomy, natural history, and medicine. The "Iggeret Orhot Olam" of 1525, the most often copied of Farissol's manuscripts, was the first Hebrew work to include the explorers' knowledge of Africa, Asia, and the New World. Ruderman has masterfully reconstructed the social environment of a late fifteenth- and early sixteenth-century manuscript writer who lived in dialogue with both Christians and Jews while fulfilling commitments to the Ferrara Jewish community as elementary school teacher, cantor, "mohel," and spokesman. Speculative aspects of the reconstruction invite other scholars to continue the search for ancillary data.

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GIUDUBALDO GUIDI. *Il governo della città-repubblica di Firenze del primo quattrocento*. Volume 1, *Politica e diritto pubblico*; volume 2, *Gli istituti "di dentro" che componevano il governo di Firenze nel 1415*; volume 3, *Il contado e distretto*. (Biblioteca Storica Toscana, number 20.) Florence: Leo S. Olschki Editore. 1981. Pp. viii, 355; 353; 305.

In recent decades the political life of Renaissance Florence has been examined with an unprecedented eye for detail and with a sensitive understanding of the complex relations that link political thought and institutions with social structure. One need only cite

the works of scholars like Marvin Becker, Gene Brucker, Dale Kent, Lauro Martines, and Nicolai Rubinstein, among others in the large army of Florentine scholars, to realize how different in methodological approach the study of Florentine politics has become since the end of World War II. Giudubaldo Guidi's three-volume book on the Florentine government, while informed by these works, harks back to an older tradition in the writing of political history, one that stresses the administrative structure of political institutions. As a result, it partakes both of the merits and the shortcomings inherent in that approach.

Guidi's work is truly encyclopedic in scope. He aims at a description of the evolution of political institutions in Florence and its countryside from 1282 to 1415. In this he succeeds admirably, which is no mean feat given the complexity of the administrative system and the cumbersome bureaucratic procedures that often characterized political activity. Florentines were very cautious in introducing institutional changes, and, rather than abolish old structures, they tended subtly to alter their functions by superimposing new structures on the old. The problem facing the would-be historian of political administration is compounded by the fact that no single set of public records covers all the relevant legislation. Communal statutes, for example, often left out important aspects of constitutional law and administration. On the other hand, they also might include laws and institutions that had long before lost their significance. Guidi has performed an extraordinary service to the field in plowing through a vast array of records, ranging from statutes, communal deliberations, lists of officeholders, chronicles, tax records, and so on, to follow the changes in institutions and electoral procedures. The result is the first book to explain in a clear and systematic fashion how the Florentine government worked at a legislative and procedural level.

The first volume, entitled *Politics and Public Law*, examines the basic principles governing the working of political institutions. The author describes the geographic subdivisions within the state, the different types of legislation and deliberative bodies, the concept of citizenship, changes in the electoral system, and finally what he terms the corruption of the system in the early fifteenth century.

Volume 2 looks at the institutions that governed the city of Florence: the priorate, the chancery, the *podestà*, the various councils, judicial and military offices, fiscal offices, guilds, and others. With each of these the author describes the origins of the institution, its powers and jurisdictions at various times, and its personnel. No office is deemed too small or insignificant to be omitted or handled carelessly. Even the *banditori* receive their due.

Unlike most historians of Florence, Guidi's inter-

ests transcend the city walls. He fully realizes that to understand Florence as the center of a territorial state, one must examine its *contado* and district. This is accomplished in volume 3, which begins with a look at local political institutions and only then turns to the institutions created by Florence to govern its subjects in its dominion. Guidi strikes a good balance in his analysis of the extent and limitations of local autonomy and is sensitive to the geographic and chronological variations in the Florentine treatment of its subject areas.

The comprehensive discussion of government institutions achieved by Guidi will make his book an indispensable tool for scholars interested not only in the politics of early Renaissance Florence but also other aspects of life as well. Those interested in economic affairs, for example, will find useful references in the chapters dealing with *gabelles* or the funded debt; those interested in crime, charity, education, or other social concerns will find that institutions dealing with these questions are also carefully described.

Still, if as a work of reference this book has everything to recommend it, as a work of political analysis it is hampered by its emphasis on administrative institutions. The limits of Guidi's approach are particularly visible in volume 1, where he attempts to characterize the nature of Florentine political life on the basis of the regulations that governed the electoral system. Guidi sees the years from 1282, when the government by the priors was created, until 1328 as formative years. The period from 1328, when the electoral system by scrutiny was devised, until 1415 is one in which the previously created system is constantly improved: "In the long years that go from 1329 to 1433, the Arno city did not experience revolutionary changes, and its government improved slowly and continuously" (p. 11). This government, according to Guidi, was unique among the Italian states after the mid-fourteenth century and was essentially democratic, liberal, and devoid of notions of political class. What made possible this model system? The heart of Florentine politics, the author believes, is its elections by scrutiny, which did not allow any one institution, group, or faction to usurp political power. The result was that a variety of groups—most notably the guilds, the companies of the popolo, and the Parte Guelfa—all shared political sovereignty.

Guidi's sanguine assessment of Florentine politics can only be explained in terms of the author's attention to the procedural aspects of political life. It bears little resemblance to the faction-ridden, oligarchical system outlined by scholars like Brucker, Martines, or Dale Kent in recent years. In support of his thesis that liberalism best describes the Florentine system, Guidi cites the same factors considered by civic humanists like Leonardo Bruni: the rapid

turnover of offices, the need for more than one magistracy to vote on certain issues, equality of opportunity in officeholding, and so on. Indeed, he quotes Bruni's statement that "in all things people and liberty were the rulers" (p. 13). As Rubinstein and others have pointed out, however, the claims of Bruni and the civic humanists on these issues cannot be taken at their face value. The officials in charge of the scrutiny system had considerable latitude in selecting the names of eligible officeholders. The theoretical equality of the system could and was easily subverted to the desires and needs of factions, especially after 1382. As the author himself acknowledges, the very conservatism of the Florentines in initiating institutional changes masks important developments in the political life of Florence. The scrutiny system could exist equally well under a variety of political circumstances, as Cosimo de' Medici well knew. It is for this reason that reliance on it for political analysis makes it difficult for Guidi to explain the "corruption" of the political system in the early fifteenth century and renders his characterization of Florentine politics difficult to accept. For an understanding of Florentine politics, historians can find more illuminating guides elsewhere. For an understanding of political administration and institutions, Guidi's book will be the standard work of reference for many years to come.

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ROBERTO MANTELLI. *Burocrazia e finanze pubbliche nel Regno di Napoli a metà del cinquecento*. (Biblioteca di Storia Economica, number 4.) Naples: Lucio Pironte. 1981. Pp. vii, 445.

The loss of Naples's archives in World War II forced scholars abroad in their search for material relating to the city's history. Because Naples was a Spanish dependency in the sixteenth century, Simancas and Madrid have provided Roberto Mantelli with rich data for this provocative study of Neapolitan public finances and the development of bureaucracy. His book is based on research in the records of Gaspar de Quiroga, Spanish inspector general, who made an extended official visit to Naples from 1559 to 1564.

Visitations were one of the principal methods the Spanish crown employed to control officials in the far-flung reaches of the empire. Quiroga diligently investigated the workings of Neapolitan government and the conduct of officials, and his reports give us a unique perspective on the problem of securing accountability in sixteenth-century bureaucracies. Corruption was rampant, and the development of clientage networks within the bureaucracy seriously hampered the business of government. In

fact, an unstated purpose of these periodic visitations was to intimidate officials into performing their jobs well. The presence of the inspector certainly inspired fear, but Mantelli concludes that it seems to have had little lasting effect in increasing either the efficiency or honesty of government officeholders.

The inspector's reports, however, were enormously useful in informing the crown about the economic conditions in Naples, about how well the apparatus of government functioned, and about areas needing reform. Mantelli uses Quiroga's reports to analyze such diverse areas of government activity as the sale of offices, the postal system, guilds, prisons, taxation, and the public debt. He illustrates how each of these areas was but a variation on the common themes of bureaucracy and public finance.

Since we already know much about Naples in the seventeenth century from the studies of Coniglio, Villari, and De Rosa, Mantelli's book is an all-the-more-welcome venture into the largely uncharted waters of the sixteenth century. The reader could wish for a more extensive treatment of how Naples fitted into the broader context of Habsburg imperial policy, a perspective only hinted at in the book's concluding chapter. Spain suffered a financial crisis in the mid-sixteenth century and made heavier and heavier tax demands on its possessions. Thus, perhaps the most crucial function of visits from the standpoint of the crown was to increase revenues owed to Spain. Mantelli's study would seem to confirm Chabod's observation for Milan that in this period the most significant ties uniting the Spanish empire were financial ones.

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SALVATORE CAPONETTO. *Aonio Paleario (1503–1570) e la Riforma protestante in Toscana*. (Studi Storici.) Turin: Claudiana Editrice. 1979. Pp. 255. L. 7,800.

Though more of a summary than a distillation of a lifetime of research in the religious history of sixteenth-century Italy, Salvatore Caponetto's study of Aonio Paleario seems destined to become the standard introduction to the life and times of this minor figure of the Italian Reformation. Caponetto's success lies in his skillful blending of the details of his subject's biography with the ideas and concerns of contemporary Italian reformers, churchmen, and intellectuals. The result is a clear and slightly old-fashioned account based mainly on the literary remains of the period.

Born in Lazio, the young humanist spent his early years as a student of Latin literature in Rome,

leaving that city after the sack of 1527 for university study of Aristotle and Lucretius at Siena and Padua. His first major work, inspired by these studies, was a long Latin poem on the immortality of the soul. Influenced early by Erasmus, Paleario campaigned for church reform and peace in Christendom. These attitudes found a congenial response from Florentine thinkers when, in 1538, Paleario married and settled in Colle Valdelsa in Tuscany. His association with Florentines of Protestant leanings and outspoken criticism of the hierarchy caused his first arrest and trial for heresy in 1542. Though the charges were eventually dropped, Paleario attacked the Inquisition in his apology, which he published only in 1552.

The reformer continued his drive for church unity, calling in 1544 for a general council of the representatives of all lay rulers to bring about reconciliation and reform. At the same time he penned a famous letter (edited here in the appendix) to Swiss and German reformers, including Calvin, Bucer, and Luther, asking for aid in this project. Employed as a Latin teacher, he next moved to Lucca, there to compose a panegyric to the glories of that city and a curious treatise on family life, arguing for the independence of wives from the tyranny of priests and husbands. Called to Milan as a professor of rhetoric in 1555, Paleario worked at the completion of his magnum opus, a statement of religious belief and program of church reform, *Actio in Pontifices Romanos*. With the election of the Dominican inquisitor Ghisleri as Pope Pius V in 1566, Paleario's case for heresy was reopened and he was transported to Rome. There, after two years of interrogation, Paleario confessed in writing to support for the Lutherans and criticism of the Inquisition. In a rare moment of defiance, he refused to abjure his heresy in public and in July 1570 was hanged and burned for his beliefs.

Caponetto has drawn a portrait of Paleario as a gentle Erasmian defending freedom of conscience, the right to debate, and Christian teaching to bring about reform. A reformer whose values were formed in the Italy of Bembo and Contarini before the convening of Trent, Paleario was fatally out of phase with the Roman church of the Counter Reformation. He was to pay for this anachronism with his life.

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CARLO M. TRAVAGLINI. *Il dibattito sull'agricoltura romana nel secolo XIX, 1815–1870: Le accademie e le società agrarie*. (Istituto di Storia Economica.) Rome: Università degli Studi. 1981. Pp. 268.

Carlo M. Travaglini's thorough if rather narrow monograph adds another page to the debates on

agrarian development in nineteenth-century Italy, in this case focusing on the agrarian academies and associations that appeared in the Papal States—or more accurately in Rome—before 1870. The starting point was provided by the radical albeit gradualist reform proposals put forward, but never implemented, by the government of Pius VII in 1802. These proposals envisaged a restructuring of the prevalent latifundist economy of the Roman countryside by means of the compulsory introduction of peasant emphyteut leases. This unrealized project was enthusiastically re-examined shortly after the accession of Pius IX, not in a fit of radical reformism but rather in a desperate attempt to put right at least part of the administrative chaos inherited from Gregory XVI. The threatened ecclesiastical and latifundist possessors had already played their obstructionist cards before academic debate was swept aside by the events of the Roman Republic, and, in Travaglini's view, the radicalism of republican reform proposals effectively removed the question of agrarian development from the agenda of subsequent papal governments. Although there was some revival of discussion in the final years of the Papal State, this was very much the result of private interests and private initiatives and followed a narrowly technical line that studiously avoided those questions of property rights and the balance between private and public good that had been implicit previously.

To break off in 1870 leaves things very much in the air and reflects the lack of a wider frame of references against which to set the analysis. Although Travaglini succeeds in revealing the different interest groups that found expression in the course of these discussions, there is too little reference either to comparable debates elsewhere on the peninsula or to the changes occurring within Italian agriculture during the period. The author laments the absence of detailed studies of similar agrarian institutions in other regions, yet his own study suggests that these essentially academic debates may not necessarily be the best way of coming to grips with either the practical or the political problems posed by agrarian development in these years.

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CLARA M. LOVETT. *The Democratic Movement in Italy, 1830–1876*. Cambridge: Harvard University Press. 1982. Pp. x, 285. \$27.50.

This is an important book, both for its methodology and its subject matter. It analyzes and discusses not only the ideological conflicts of the Risorgimento but also, more generally, the dialectical process at work within leftist ranks. Continuing research on

nineteenth-century Italian ideologues begun in her studies of Carlo Cattaneo and Giuseppe Ferrari, Clara M. Lovett has written a seminal work on the relationship between revolution and legal political activity in Italy. Her prosopographical study traces the careers of 146 militants through the insurrectionary phase of the Risorgimento and during the years immediately after unification. It combines social, political, and intellectual history to give an analysis of a momentous period in the emergence of modern Italy.

The Risorgimento had achieved Italian unity by 1861 as the result of a marriage between Cavourian diplomacy and the surviving insurrectionary élan and appeal of the 1840s revolutionists. But as divisions persisted and intensified between the two factions after unity, the democratic—and in some cases still republican—left, inside and outside Parliament, continued to be concerned with the ideals of secularism, political equality, and social justice that had inspired their struggle for independence and unity, while the heirs of Cavour sought to build a state.

Lovett traces the activities of her 146 “thinkers, propagandists, and militants” from the early 1830s to 1876, when the left came to power in the parliamentary upset of that year, replacing the Cavourian right. This changing of the guard, however, did not usher in far-reaching reforms, and the left fractured into two groups: the historical Left, those in power, and the left-wing opposition, or the Extreme, which arose in 1877 to challenge the government. As the revolutionary phase and the Risorgimento itself became past history, its leaders were metamorphosed, for a monarchical Italy had little place for republican and socialist heroes. Mazzini, Cattaneo, Ferrari, Pisacane, and Garibaldi were remembered rather as nationalists than as social reformers.

Lovett concludes, after an analysis that goes to the heart of what may be called the moving ideals of the nineteenth-century revolutionary movement in Italy, that “the democrats who remained active after 1876 passed on to their disciples and admirers some of the passion for political equality, secular culture, and social justice that had guided their own actions in the Risorgimento” (p. 238). But as *trasformismo* tainted the left in Parliament and the hoped-for changes did not materialize, new centrifugal forces inspired by Bakunin and Marx further split the old Risorgimento left. This fragmentation, according to Lovett, has had far-reaching impact, for it “explains in part why Italy's political culture has produced a myriad of intellectually lively non-Marxist democratic groups but no important secular democratic party with a social base in the middle class” (p. 239).

This is a book that offers much more than its title implies. It is a penetrating analysis of the leftist

persuasion in general, based on thorough research in primary sources.

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ANTONIO CARDINI. *Stato liberale e protezionismo in Italia, 1890–1900*. (Saggi, number 207.) Bologna: Il Mulino. 1981. Pp. 342. L. 20,000.

Antonio Cardini is well qualified to write this impressive study that fills a gap in a most misunderstood subject and provides an understanding of the political and economic dichotomy in Italy since unification. As a member of the department of law at the University of Siena in Florence, he teaches political science and is actively involved in doing research on the relationship between politics and the Italian economy. He has published a number of articles and books on the subject, the best known of which are *The Search for a New Bourgeoisie in the Cultural and Political Debates of the 1890's* (1978) and *The Italian Economic Culture of the Early 1900's and the Era of Imperialism* (1981).

Cardini hopes this study will prove to be a major contribution toward understanding a problem that up to now has been overlooked and misunderstood: the political function of the Italian economy since the era of the 1800s, especially the role of criticism in stimulating deliberation and denunciation of the ruling class on matters of state since the Risorgimento. Cardini is especially concerned with Italy's economic problems and their consequences since unification and the numerous crises they presented at the end of the century. The question of protectionism presented a serious problem. The debates that ensued between the ruling elite and the Italian masses undoubtedly became a classical confrontation that revealed the difficulties and weaknesses of both the Italian parliamentary government and of a real Italian class structure.

The problems of a forced economic development on the Italian masses are examined from a number of angles. Different points of view are presented, thoroughly analyzed, and described opposite the political consequences and their impact in terms of personal consequences, the growth of democracy, and a state administrative structure. Cardini supports his arguments by referring to many liberal Italian economists and political sociologists such as Antonio de Viti de Marco, Vitero Pareto, and Gaetano Mosca. The debates on economic politics were undoubtedly a forum for extensive criticism relating to the objectives of Italian capitalism, the nature and character of the bourgeoisie, and the relationship between state and society. All these issues are juxtaposed within the dominion of pure liberalism. These debates are perhaps the most

authentic in Italy on the ideology of liberalism. Despite the controversy and its precarious existence in Italy, the concepts and ideas suggestive in economic liberalism were able to survive a rather shaky existence.

The book will probably be a wearing experience for most readers who are not familiar with Italian authors. They may experience some confusion and frustration with the flowery style that detracts from its clarity. A more positive note is that Cardini has made a much needed contribution to the literature on a most overlooked problem. The book is very informative. It is a well-researched and well-documented piece of work that provides both necessary groundwork and a welcome impetus to further investigations. The author knows his material and his arguments are persuasive. This work is highly recommended not only to scholars but also to those interested in a difficult period in Italy after unification to the end of the nineteenth century.

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ALBERTO AQUARONE. *L'Italia giolittiana, 1896–1915*. Volume 1, *Le premesse politiche ed economiche*. (Storia de'Italia dall'Unità alla Repubblica, number 3, part 1.) Bologna: Il Mulino. 1981. Pp. 464. L. 12,000.

Alberto Aquarone, a senior Italian historian of established reputation currently teaching at the University of Rome, has long been planning a multivolume history of Italy during the Giolitti era. Unfortunately, this first volume extant does not easily lend itself to critical review, for it is in effect an extended *introduction* to what the author planned to be his second volume, dealing with the era's economic aspects. We have here then, a prime example of a publisher's seemingly inexplicable decision seriously interfering with dedicated scholarship.

Since the volume is meant essentially as a preface to a subsequent work, it is more factual than interpretive. It is, however, extremely well documented from a great variety of published sources, although the actual bibliography as well as the author's notes on archival resources have not been included. Aquarone nonetheless acquits himself well. His introductory chapter, which limns Italy's economic system and its problems at the turn of the century, reveals a masterful grasp of his subject matter, and his concluding essay on the South and the problem of emigration is one of the most thorough and penetrating treatments of the topic this reviewer has seen. There appears to be only one flaw of consequence in the work: at times the author evidently works from the premise that Italy was a great power, a supposition that renders comparisons between Italy's economic problems and growth and

that of the French, German, and Russian economies less meaningful. But aside from this, the volume gives every evidence of being the first in a series which will prove quite valuable and will certainly provide a fresh and in-depth study of Italy's society and politics from the advent of the twentieth century through its entrance into the Great War in May of 1915. One can only hope that Aquarone's publisher will prove more cooperative in the future, for his completed work will certainly be, at the very least, required reading for all students of the Giolitti era.

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ETTORE ROTELLI, editor. *La ricostruzione in Toscana dal CLN ai partiti*. Volume 1, *Il Comitato Toscano di Liberazione Nazionale*; volume 2, *I partiti politici*. (Temi e discussioni.) Bologna: Il Mulino. 1980–81. Pp. 454; 999. L. 20,000; L. 40,000.

The two volumes introduced to the reader by the president of the Tuscan Regional Council, Loretta Montemaggi, are but a small part of an impressive, if not awesome, undertaking initiated by the council on the thirtieth anniversary of the founding of the Italian Republic. Ultimately the project will involve nine areas of study, each in one or more volumes, plus pertinent documentary compilations. The council proposes a critical examination of a number of important aspects of contemporary politico-cultural relations between state and society, evolving primarily from regional activities and experiences of the war years: clandestine activities, the maturing of political ideologies, the autonomous economic and cultural ventures, the impact of the Allied presence, and much, much more.

These two particular volumes, under the direct editorship of Ettore Rotelli, are the work of well-qualified researchers. The stage is set in the first volume by a brief introductory explanation of the nature of the Tuscan Committee of National Liberation (CTLN) as it emerged during the war. Three major components of varying length plus a concluding commentary complete the volume. The first section examines in detail the role of the CTLN in coming to grips with the realities of the problem of reconstruction. The second, of major interest to the American scholar, treats the political and economic roles of the Allies in Florence in 1944–45 through the work of the AMG. The third section investigates the emergence of new and the re-emergence of old industries in Florence during the war and in the immediate postwar years, assembling data that will prove invaluable to future researchers, especially economists. Commentary on the role of the Committee for the Reconstruction of the Province of

Florence, together with impressive statistical data, conclude the first volume.

Volume 2, *I partiti politici*, is introduced by Luigi Lotti with a definition of the scope and parameters of this inquiry; this is followed by the seven studies of the individual parties considered. These vary in length, in detail, and in investigative methodology, but, in the main, they examine the roots, nature, and step-by-step evolution of the major political parties that developed in Tuscany during the war and in the immediate postwar period (essentially 1943–48): the Christian Democratic, Action, Liberal, Republican, Socialist, Socialist Worker (including schismatics), and Communist parties.

Granted the allowances one must make for the problems inherent in compilations by committees, these volumes are invaluable to the scholar studying the history of the republic. No less important are the detailed footnotes, particularly those in volume 2, in themselves a bibliography of major importance for future research. The same must be said of the detailed statistical data incorporated in every one of the essays, material very nearly impossible to obtain elsewhere.

These volumes and others like them emerging from comparable regional undertakings will serve well the needs of the historian, the political scientist, the economist, and the sociologist, and they should be made available in all of our research centers.

GEORGE A. CARBONE
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HANSJAKOB STEHLE. *Eastern Politics of the Vatican, 1917–1979*. Translated by SANDRA SMITH. Athens: Ohio University Press. 1981. Pp. 466. Cloth \$26.95, paper \$13.50.

This is a translation of Hansjakob Stehle's updated work, *Die Ostpolitik des Vatikans, 1917–1975*, which received wide attention when first published in 1975. Using diverse archival documentation, buttressed by interviews with unlikely or generally inaccessible historical actors, the author, an experienced German journalist with distinguished service in Eastern Europe, unravels the mysterious story of diplomatic cooperation and jousting between Moscow and the Vatican in the 1920s and discusses current East European relations with the Roman curia. He also argues a point of view. Stehle insists that although the ultimate goal of Vatican diplomacy is pastoral, the popes nevertheless think in terms of "pastoral *Realpolitik*." "Pragmatic" considerations always exert stronger influence on the Vatican's diplomacy than "considerations of principle." He regrets that, although at first wisely pursuing a Rapallo-type *Realpolitik* with the Soviets, the Vatican eventually became impatient and unrealistic. It was

a mistake, in Stehle's view, for the Vatican to establish and support an illegal underground Catholic church in Russia. It was even a greater error for Pius XII to pursue "cold war" policies after World War II. Only by adopting a policy of detente under John XXIII and especially by the efforts of Paul VI and Archbishop Casarolli, who is widely quoted, did the Vatican return to its senses. Thus, it became possible for the Vatican to salvage what Stalin had not completely destroyed and also to establish for Eastern European churches "normal," that is, reasonable if not free, conditions for pastoral activity.

Stehle argues an appealing thesis that philosophically is rooted in the belief that the only alternative to "accommodation" with the Communists is self-destruction. The author's view is based on the proposition that the church's institutional organization is more important for its self-preservation than its ability to exercise pastoral and teaching functions.

This double purpose of Stehle's—to analyze while advocating a policy alternative—elicits for the book a theoretical hypothesis but provides a myopic view of the examined epoch and hurts the historical analysis itself. In this brief review I can focus only on some elements of this most interesting and important book. First, although Catholics and non-Catholics alike will continue to criticize Pius XII for failing to give clear directions during World War II, Stehle's picture of this complex pope is not only akin to Rolf Hochhuth's literary fantasies but most likely gravely unjust. Stehle condemns the pope for issuing the anti-Communist decree of July 1, 1949, which—he gleefully reports—embarrassed and hurt East European clergy, but he fails to mention that this decree was primarily aimed at West European, especially Italian, Catholics who seemed to need a shock treatment to restrain them from embracing Italy's Communist party. Without justification, Stehle makes this decree of 1949 responsible for the suppression of the church in the Soviet Union and elsewhere rather than Stalin's policies starting in 1946. In my view, furthermore, Stehle is very inconsistent. For example, he praises the Vatican for following Weimar Germany's example of Rapallo, but castigates Pius XII for what he perceives as the Vatican's imitation of American Cold War policies toward Eastern Europe. He suggests that the pope followed the political direction of the United States, but he offers no proof. Again, the author finds the Vatican wanting for not condemning the Nazis in a more direct way, but he considers such actions against Stalin and the Communists ill advised.

Similarly, as if purposefully to provoke controversy, Stehle discusses the Vatican's detente with Communist governments in a rather partisan way. No observer can deny that in some ways the policy of negotiation helped the church, even in Latin-rite regions of the Soviet Union. Stehle does not, however, differentiate between detente with the Soviet

Union and detente with Eastern Europe, and he also makes rather extravagant claims for the policy. Furthermore, he neglects important historical dimensions to explain how the policy of negotiation came about and what its relative value to individual churches was. In his examination, Stehle completely disregards the most crucial fact of all, namely, that in Eastern European states, even in Soviet Lithuania, churches survived not because of detente but because of their own strength. *Ostpolitik*, similarly, produced easier normalization of church-state relations in those countries in which the church was stronger, either for domestic reasons, as in Poland, or for international reasons, as in East Germany. The Vatican's diplomacy was not very helpful where the church was weak, as in Romania, or even in Hungary, where Catholicism has become very secularized. Another factor the author disregards is the domestic and international needs of the Soviet and East European Communist regimes, which invited rapprochement and, for some, even made it desirable.

Stehle's rejection of "catacomb" activity and "martyrdom" as a method for church survival has no foundation in historical development. The church has always thrived by using both—confrontation tactics to remain faithful to principles and diplomacy to produce pragmatic solutions. The author mentions criticisms of confrontation tactics by Eastern European clerics but omits even stronger criticisms of detente policies by Soviet Catholics. In the Soviet Union these policies created an impression of papal legitimization of the suppression of the Ukrainian Uniate Church. They also helped to drive a portion of Lithuanian Catholics into the underground. The rise of the Catholic dissent movement was largely caused by the concern that the Vatican would unwittingly help to destroy Catholicism in the Soviet Union through organizational accommodation with the Soviets. Indeed, those religious groups under Communism are stronger that care less for organization and more for missionary-pastoral activities.

The volume's translation is in general smooth and idiomatic, but the reader will not understand why the translator kept German toponymy and German transliteration from Russian in a book in English and for American readers. Stehle is a persuasive writer, but he gives a rather one-sided view of the virtues of Vatican *Realpolitik*. History certainly does not prove it nor does this otherwise excellent and informative book.

V. STANLEY VARDYS
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ROBERT BIRELEY. *Religion and Politics in the Age of the Counterreformation: Emperor Ferdinand II, William Lamormaini, S.J., and the Formation of Imperial Policy.*

Chapel Hill: University of North Carolina Press. 1981. Pp. xiii, 311. \$28.00.

This is Robert Bireley's second book on the interplay of religion and court politics in the Thirty Years' War. His earlier study of Elector Maximilian I's Jesuit confessor, Adam Contzen, stressed the decisive influence of religious motives and ideology on Bavarian diplomacy. In this sequel he has examined the role played by Emperor Ferdinand II's confessor, William Lamormaini, S.J., in the so-called Danish and Swedish stages of the conflict (1624–35). In Bireley's judgment, Lamormaini was instrumental in fortifying Ferdinand's faith in divine guidance and intervention, thus inspiring him with a "profound sense of mission" (p. 14) that gave precedence to the satisfaction of confessional interests at the expense of Austrian and Habsburg dynastic concerns.

The book is organized chronologically, and the author proceeds step by step through the issues and choices that confronted the Hofburg. Time and again, Lamormaini emerges as a Catholic "hardliner," who consistently argues that any compromise to avert conflict with the emperor's Protestant Habsburg subjects or imperial vassals constitutes a renunciation of faith in divine intervention and that God will surely reward those who do his work in eradicating heresy in both the Hereditary Lands and the Holy Roman Empire. His faith was by no means shared by the rest of the court. In fact, the most illuminating part of the book is its portrait of the numerous factions that opposed Lamormaini, including the majority of the emperor's ministers, many of the other churchmen at court, the Spanish, and on occasion even the papal and Bavarian envoys. What emerges is the image of a cautious Habsburg court that generally but unsuccessfully opposed the Edict of Restitution as a dangerous provocation of Germany's Protestant moderates and that pressed hard for compromise with the Swedes and Saxons until its ultimate victory over Lamormaini in the Peace of Prague.

The book itself is thoroughly researched, resting largely on extensive archival material unearthed in Rome, Vienna, and Munich. Its portrait of Lamormaini's motives, role, and influence on Ferdinand is on the whole convincing. Yet the author's most important contribution lies—as the book's main title implies—in its exposition of the interplay of politics and religion of the court as a whole, a much broader and more complex picture in which Lamormaini and even Ferdinand are often lost. Lamormaini's "disappearance" from parts of the text is both necessitated by the need to discuss the important role played by other individuals and unavoidable on those issues where surviving material has not given us an intimate glance at the confessor's contribution. It is more difficult, however, to

understand the slight attention paid to Ferdinand, who remains very much an enigma through most of the book. The book suffers from a surfeit of detail that makes for an occasionally boring text, despite a clear writing style and the inclusion of helpful introductory sections in each chapter. The author's concern for detail frequently comes at the expense of reflection and generalizations that might afford the reader a better overview of the significance of what is happening. For example, the conclusion does not provide a recapitulation or synthesis of Lamormaini's contribution to the politics of the Habsburg Counter Reformation under Ferdinand II. Especially disappointing to this reviewer was the author's failure to make the obvious comparison of Lamormaini with the leading French clerical figures such as Richelieu, Father Joseph, and especially those members of the so-called *Devôt* party with whom he had so much in common. In fact, Bireley's evidence makes it clear that Lamormaini, like the *Devôt* party, sought the subordination of dynastic and state interests for the sake of a confessional detente with his sovereign's natural enemy.

This oversight cannot, however, obscure the author's contribution in presenting us with a solid exposition of the interplay of secular and religious interests in policymaking during the Thirty Years' War.

CHARLES INGRAO
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JOHN O. IATRIDES, editor. *Greece in the 1940s: A Nation in Crisis; Greece in the 1940s: A Bibliographic Companion*. Compiled by HAGEN FLEISCHER and STEVEN BOWMAN. (Modern Greek Studies Association Series, number 4 and 5.) Hanover, N.H.: University Press of New England. 1981. Pp. xvi, 444; viii, 94. \$35.00; \$15.00.

In 1969, I reviewed for the Paris *Revue historique* a publication entitled *Greece since the Second World War*, presenting the papers read at a University of Wisconsin symposium, held on April 10–12, 1967, on the occasion of the twentieth anniversary of the Truman Doctrine. I made the remark that the atmosphere of that gathering was rather right wing. General Van Fleet, a participant, called the Greek Communist guerrillas "international gangsters."

The present book is also the result of a symposium that dealt with the same subject. But, as one organizer remarked, a participant said: "At times I felt that you had invited all the Southerners after the American Civil War to explain to us why they lost, while you never thought of inviting the Northerners to tell us how they won!" (p. ix).

If the left-wing orientation of the symposium was evident, however, one must admit that it was on a much more scientific level than the 1967 one.

Because of the number of the contributing scholars and the quality of their sources and analyses of Greece and communism, a certain right-wing fanaticism still present in Greece will find it more difficult to express itself in the future. I refer to the fanaticism of those who have doggedly persisted in calling the Greek Civil War a "bandit war" and the Communists bandits, adding that the term "civil war" was not applicable to the Greek case, because the Communists were receiving outside help. But one ought to know that the rule in a twentieth-century civil war is precisely this foreign presence and often on both sides, the best example being the Spanish Civil War (p. 196).

The encouraging point about this symposium is that it has proven that today there are many scholars—mostly outside Greece, but in any case who are Greeks or know modern Greek—that study twentieth-century Greek history, while in Greece the Second World War and its aftermath still remain outside the realm of university research.

John O. Iatrides has done a very good editing job. His three introductions and his own paper on the Civil War are enlightening. He has regrouped the contributions in two parts, the first part covering the Second World War period and a second part focusing on the liberation of Greece and the Civil War. Generally speaking, the papers in the first part are based, for obvious reasons, on more archival material and consequently are more solid than the ones in the second part. Unfortunately, no participant has been able to use Greek archives.

Best examples of serious scholarly work, based on British and German wartime records, are the papers of John Hondros on the Greek Resistance, 1941–44, and of Hagen Fleischer on the problem of collaboration with the occupants of certain Resistance organizations during the same period. The Greek Resistance against the Axis powers has not yet been studied as a whole by scholars, and the very poor book *La Résistance grecque* that a novelist, André Kedros, had published in Paris in 1966 is unreliable. Both of the above-mentioned contributors promise a forthcoming book each on the subject.

Fleischer, in particular, handles in a skillful way the very delicate and complex problem of collaborationism within the ranks of Greek resistance. This is still a very hot issue in the daily press of Greece, each side accusing the other of collaboration. The study proves that many anti-Communist resistance groups finally collaborated with the Italians and the Germans. Nevertheless, some questions remain unanswered: Taking into account that the resistance groups were waging a twofold struggle—a civil war against one another and an anti-Axis war—what would have happened if, let us say, the Soviets instead of the Germans had invaded Greece? Would not the Greek Communist guerrillas tend to collabo-

rate with them against the anti-Communists? And, second, is there not evidence of EAM collaboration in Macedonia and Thrace with the Bulgarians, in September–October 1944, before the end of the war, at the time of the Red Army advance into Bulgaria? These questions underline the fact that the first phase of the Civil War took place during the Axis occupation and that this internal strife seemed to many Greeks as important as foreign occupation. In the villages of the Peloponnese, I have noticed that the peasants do not clearly distinguish between an Axis occupation period and a postwar Civil War period. For them there is only a civil war period extending from 1943 to 1949, in which one after the other, Italians, Germans, British, and Americans have interfered.

The *Bibliographic Companion*, as a separate volume, contains two very useful bibliographies: one by Hagen Fleischer on Greece under Axis occupation, the other on Jews in wartime Greece by Steven Bowman. The latter particularly is a valuable attempt to put the documentary basis to a future long-awaited systematic scholarly study of the Greek Jews during the Second World War.

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NIKŠA STANČIĆ. *Hrvatska nacionalna ideologija preporodnog pokreta u Dalmaciji: Mihovil Pavlinović i njegov krug do 1869* [Croat National Ideology of the Movement for National Revival in Dalmatia: Mihovil Pavlinović and His Circle to 1869]. Summary in German. (Centar za Povijesne Znanosti, Odjel za Hrvatsku Povijest, Monografije, number 11.) Zagreb: Sveučilište u Zagrebu. 1980. Pp. 398.

Dom Mihovil Pavlinović (1831–87), a priest and political thinker, was the leading protagonist of the Croatian national revival in Dalmatia. The expansion of Venetian holdings on the eastern shores of the Adriatic defined the modern frontiers of this ancient province and disrupted its bonds with the rest of Croatia. Venice neglected the once powerful Dalmatian cities, which were, in any case, no longer the centers of commercial enterprise and Croat literary activity. The dormition of Dalmatia lasted for centuries, coming to an end only with Napoleonic occupation and the subsequent passing under Habsburg rule. Like the rest of southern Europe, Dalmatia was noted for its local color and poverty. In the central portion of Dalmatia, which is the locale of this book, there was a sharp contrast between the cities, the largely coastal hubs of Italianate urban culture, and the primeval countryside—either the tenurial lands in the vicinity of the cities and on the Adriatic islands or the vast hinterland, the Morlacchia of Venetian parlance that was ruled

by the Ottomans until the eighteenth century and whose inhabitants lived in patriarchal agelessness.

The point of Nikša Stančić's splendid and finely erudite book is that the "popular intelligentsia" of the hinterland, mainly priests like Pavlinović, were the motor force in the re-Croatification of Dalmatia, wresting it from the control of a denationalized segment of the bourgeoisie that was fast metamorphosing into a detachment of Italian irredentism. Stančić has produced a masterful study of the generation of the 1850s and 1860s—the group that he calls the "basic revivalist generation." He offers a detailed representation of the ideas and activities of these men, especially Pavlinović and his group, following them from their early interest in the Illyrianist movement, through a brief identification with the Serbian national ideology of Vuk Karadžić, to their adoption of the two principal Croat national ideologies of Dalmatia—a version of Rački's Yugoslavism and, slightly later, Pavlinović's form of integral Croatianism (1869).

Stančić found his sources in the main archives as well as in unfrequented collections. His patient detective work is an inspiration, and he was rewarded in 1969 with the discovery of Pavlinović's long-lost national program "*Hrvatska misa*." Most important, the publication of Stančić's work will end the discussions about the social origins of revivalist intelligentsia. Stančić's "sociogram" of the men who shaped the Croat national revival in Dalmatia documents a distinction between the urban and "popular" intelligentsia, a discrimination that Pavlinović also recognized (p. 229). According to Stančić, "Pavlinović and the other members of popular intelligentsia could rely only on the [village-based] stratum that was on a material upturn, but that did not yet enter the urban society," that is, on the group "that did not become [nationally] alienated under the influence of the city but retained its Croat ethnic consciousness" (p. 137).

The social origin of revivalist leaders does not, however, tell us much about the origin of national ideologies among the Dalmatian Croats. Stančić indirectly recognizes this important problem by noting that, unlike the Serb peasantry of Dalmatia (whose point of identification was membership in the Eastern Orthodox church), the Serb bourgeoisie "possessed a developed Serb national consciousness" even before the 1860s (p. 306). The Croat peasants of Dalmatia, however, were far more nationally conscious than the Croat bourgeoisie. Stančić's book underlines the need for a theory on the *mechanics* of national integration among the South Slavs, a problem that cannot be solved by social history alone. The lack of such a theory explains Stančić's frequent recourse to external and historical "influences," which is the traditional (and increasingly unsatisfying) way of resolving this problem.

Stančić's present volume ends rather abruptly with Pavlinović's summary treatise of 1869. It is to be hoped that Stančić will continue his research with new monographs on the history of Croat and Serb national ideologies in Dalmatia. His work and that of Trpimir Macan, who has written an equally excellent biography of Miho Klaić (1980), have made this one of the liveliest fields of Croatian historiography. One promising area of investigation is the circle of Ante Kuzmanić, a group whose vast influence is largely skirted in this study.

IVO BANAC
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BARBARA K. REINFELD. *Karel Havlíček, 1821–1856: A National Liberation Leader of the Czech Renaissance*. (East European Monographs, number 98.) Boulder, Colo.: East European Monographs; distributed by Columbia University Press, New York. 1982. Pp. 135. \$17.50.

For nearly two hundred years, journalists in East Central Europe have played an important part in the political and cultural life of their nations. This results in part from restrictive governmental systems that have limited the opportunities for organized political expression. Unfortunately, only a few books and articles have explored the way in which the journalist has exercised this influence.

This slim volume, essentially Barbara K. Reinfeld's 1979 Columbia University dissertation, is the first English-language treatment—except for an unpublished Indiana University dissertation by Thomas G. Pešek (1970)—about the political life and thought of a journalist who was one of the leading figures of the Czech national movement in the first half of the nineteenth century. Karel Havlíček-Borovský edited three periodicals that represented the liberal democratic Czech position during the stormy period surrounding the 1848 "springtime of revolutions." Reinfeld describes his writing on each paper, observing that it was Havlíček who introduced political content into Czech journalism. She also notes that Havlíček thought that providing enlightenment was of equal importance to the reporting of news, a concept that Czech journalism follows today, although under quite different circumstances. The author is sensitive to the national complexities of the 1848 revolution and observes that Havlíček's liberalism did not extend to granting Slovaks the same right of national identity that he and his fellow Czechs sought. Although the author does present an easily understood description of Havlíček's attitudes as they were changed by new political circumstances, nearly

half of the book is devoted to a description of the Czech experience in 1848–49, which is more thoroughly examined in Stanley Z. Pech's *Czech Revolution of 1848* (1969).

While Reinfeld devotes part of her conclusion to Havlíček's impact on Czech national thought since his death (especially on Thomas G. Masaryk, the founder of the modern Czechoslovak state), it is distressing that there is little consideration of Havlíček's influence specifically as a journalist. Yet this is precisely why Havlíček has been considered an important figure. A basic problem is that the author does not present criteria for measuring this influence. She offers several circulation figures and notes the increases that followed Havlíček's becoming editor. Is this a simple cause-and-effect relationship? The number of published copies turns out to be very similar to the number of persons identified as being Czech patriots in the 1840s (see Miroslav Hroch's "The Social Composition of the Czech Patriots in Bohemia, 1827–1848," in Peter Brock and H. Gordon Skilling, eds., *The Czech Renaissance of the Nineteenth Century* [1970] pp. 33–52). Did the circulation figures reflect Havlíček's influence as a journalist, or did the existence of his paper and the subjects it discussed provide nationally conscious Czechs with an opportunity to assert their nationalism by subscribing? The latter conclusion is supported by Havlíček's avowed intention of breaking Czech dependence on German culture, a policy that he brought to his paper by using many foreign sources other than German and by employing colloquial Czech rather than more literary forms, which had characterized earlier publications.

Havlíček's journalistic influence should be seen as one of defining an acceptable national agenda for the small group of people already active in the national movement. His political influence, redefining the concept of a nation in political terms, proved to be more influential after his death than during his lifetime.

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ALEXANDER YANOV. *The Origins of Autocracy: Ivan the Terrible in Russian History*. Translated by STEPHEN DUNN. Berkeley and Los Angeles: University of California Press. 1981. Pp. xvi, 339. \$19.95.

The Origins of Autocracy is an audacious book because it seeks to dismantle what Alexander Yanov believes to be simplistic interpretations, both Soviet and Western, of the nature of Russian history. To Soviet historians, who labor under the *vyskazyvaniia* (authoritative statements) of Marxism-Leninism and try to force the Oprichnina into a model of absolutism,

and to Western scholars, who insist upon seeing Russia as an example of despotism, whether patrimonial, Byzantine, or Tatar, Yanov offers the explanation of autocracy. The book is divided into three parts: an analysis of historical models, the absolutist century from Ivan III to the Oprichnina, and the historiography of the Oprichnina; although highly polemical, it offers trenchant critiques of the scholarship of Ivan the Terrible.

For Yanov, Russia until the Oprichnina was a variant of Western absolutism—that is, a government that recognized inherent political, social, and economic limitations on the exercise of power. He believes the historic function of Western absolutism, as the cultural school of mankind, was to lead Europe toward democracy—or what Montesquieu saw as the separation of power—even though this path led through the muck of religious wars, revolution, slavery, and even terror. If many a monarch, such as Louis XI, sought the ideal of despotism—that is, power without limits—only Ivan the Terrible succeeded during the Oprichnina. The Oprichnina was a revolution from above; it halted the process of Europeanization, radicalized Russia's economic and political institutions, formed a "new class" on the basis of terror, and led to a separation of the functions of power. Yet Russia did not become a model of despotism. Despotism failed because of the absolutist traditions, but the Oprichnina so deformed these traditions that the result was autocracy. Henceforth, Russia has oscillated between rigid patterns of terror with new and savage elites and relaxed phases of "thaw," reform, and stagnation—only to repeat the cycle anew with feverish change. The fatal moment came when Ivan turned his back upon Europeanization and launched the ill-prepared Livonian war.

As a model of history the book is flawed. Despotic Byzantium understood the limitations of power, and absolutist Europe saw the eventual rise of Nazism. Nowhere does Yanov fully explain why Ivan succeeded, and comparisons of Ivan, Peter the Great, Lenin, and Stalin simplify the historical context beyond recognition. Yanov's analogy of the Non-Acquirers and Josephites as Russian versions of Protestantism (supporters of secularized church lands and a free peasantry) and Catholicism (adherents of state power and an enserfed peasantry) ignores the Byzantine roots of the Russian religious debates and the fact that both Prussia and Austria fostered serfdom. Nor will Yanov find the nascent Russian bourgeoisie among the so-called protobourgeoisie of the wealthier strata of peasant society. Ivan's Livonian war is not extraordinary when one considers not only the policies of Ivan III but also the military function of the European state. And the creation of a Russian service nobility parallels similar developments in Prussia.

Nevertheless, this is an important book if only because it forces historians to re-evaluate their models of Russia and what constitutes a meaningful comparison with European history. As a genre of historical writing, it takes an honored place in the long and continuing Russian debate on the "curled questions" of the meaning of Russian history.

LAWRENCE LANGER
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OREST SUBTELNY. *The Mazepists: Ukrainian Separatism in the Early Eighteenth Century*. (East European Monographs, number 87.) Boulder, Colo.: East European Monographs; distributed by Columbia University Press, New York. 1981. Pp. 280. \$20.00.

Because Byron devoted one of his long romantic narratives to Ivan Mazepa, he is the only early Ukrainian leader known to most educated speakers of English. In the contemporary Ukrainian pantheon, however, Mazepa occupies a relatively minor place. His movement for Ukrainian independence came late in his life and he died in the midst of defeat. As suggested by the number of times his name was chosen as a pseudonym by recent Ukrainian nationalists, Hryhor Orlyk, Mazepa's successor, attained more fame among his own compatriots. Yet Orlyk, a recent arrival in the Ukraine, was the descendant of a distinguished Czech family. Very important for his place in history was Orlyk's composition of a remarkable memorial, which (appearing in English for the first time) constitutes the larger portion of the extensive documentary appendix in Orest Subtelny's book. It is understandable that the author devotes most of his exposition to Orlyk's twenty-year struggle to preserve the nucleus of an independent Ukraine. Nevertheless, Subtelny emphasizes, it was Mazepa's decision to conspire with Charles XII of Sweden against Peter the Great that led, ultimately, to one of the first of many Ukrainian catastrophes. This debacle included Russian suppression of the institutional center of Ukrainian autonomy, the Cossack Zaporozhian *Sich* (Host); its memory remains the core element of the Ukrainian constitutive myth.

Subtelny has not tried to produce a complete history of the Ukraine in the early eighteenth century. Military events, which usually receive most attention, are only summarized. Strategic implications for Russia, the Ottoman empire, and other East European powers are not fully clarified, although the author identifies significant factors like the virtual impossibility of a durable alliance between Ukrainians and the predatory steppe allies of the Ottomans. Instead, Subtelny concentrates on diplomacy in the social context of international developments. Versed in the history of European social move-

ments, he locates the Cossack leaders in the belated East European echo of the Western defense of estate privileges. Unfortunately for the aristocratic strata involved, this opposition to centralization reached the Russian borderlands just in time to encounter Peter's overwhelming drive for a centralized autocracy.

Subtelny is equally good in examining the social background of his protagonists, whether in the biographical mode or in broader consideration of the relationship of the Cossack *starshyna* (officer elite) to the Polish *szlachta* model. At the same time, he emphasizes that symbolic politics were at least as important as elite interests. His treatment of verbal symbols (particularly the shifting use of "Ruthenian," "Ukraine," and "Little Russian" by the Cossacks themselves) might have been more explicit. But analysis of the primary body of symbols, related to the Orthodox religion, is excellent. Altogether, this book is an excellent example of how sophisticated concepts can be skillfully integrated with meticulous monographic research.

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I. S. DOSTIAN. *Russkaia obshchestvennaia mysl' i balkanskii narody: Ot Radishcheva do dekabristov* [The Russian Social Idea and the Balkan Peoples: From Radishchev to the Decembrists]. Moscow: Nauka. 1980. Pp. 325. 2 r.

Since the late 1950s I. S. Dostian has published several well-documented studies concerning the relations of Russia with the peoples of the Balkans at the beginning of the nineteenth century. Her most recent book consists of four extended essays on Russian social thought and the peoples of the Balkans. The first of these essays discusses "Russian social thought and Balkan problems at the end of the eighteenth century and in the period of the Napoleonic wars"; the second, the dissemination in Russia of knowledge about the Balkan peoples during the first quarter of the nineteenth century; the third, "the Balkan question in the world view of the Decembrists"; and the fourth, "ideas on the unity of the Slavic peoples and notions about their ethnic common character (*obshchnost'*) in Russian social thought, 1815-1825." Dostian's essays contain a wealth of new or overlooked materials concerning what Russians then wrote and thought about the peoples of the Balkans. She has made good use of pertinent Russian publications and journals of the late eighteenth and early nineteenth centuries. She has also had access to materials in Soviet archives. Materials she has examined in the Central State Military and Historical Archive (TsGVIA) would

seem to be particularly important, for they document in detail official Russia's interest in the Balkans during the Napoleonic and Russo-Turkish (1806–12) wars and during the Serbian uprising of 1804–13 and the Greek uprising of the 1820s.

Dostian's study is above all valuable because of its discussion of how knowledge about the Balkans was collected and disseminated in Russia at the beginning of the nineteenth century. Particularly noteworthy is what she writes concerning studies made for or resulting from military and diplomatic missions. But why she included fifteen pages on Radishchev in her book is not entirely clear, for he does not emerge from its pages as a significant commentator on Balkan events and conditions. Indeed, the theme of "Russian social thought and the Balkan peoples" is only developed satisfactorily in her sixty-seven-page essay on the Decembrists. Here, however, one needs to read Dostian together with Georges Luciani's study of 1963 on the Society of United Slavs, which perceptively analyzes the intellectual currents and the Polish–Little Russian social setting that helped give birth to panslavism and ideas of Slavic solidarity among future Decembrists stationed in the Right-Bank Ukraine and Little Russia during the decade immediately following the Napoleonic wars.

Dostian's book is, however, indispensable reading for all those interested in the history of panslavism in Russia. It is regrettable that the Soviet Academy of Sciences did not see fit to provide this useful study with a bibliography and an index.

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NANCY MANDELKER FRIEDEN. *Russian Physicians in an Era of Reform and Revolution, 1856–1905*. Princeton: Princeton University Press. 1981. Pp. xvii, 378. \$32.50.

This informative and carefully researched monograph traces the professionalization and eventual politicization of Russia's physicians during the half-century of rapid social change that followed the Crimean War. The book is not a history of Russian medicine, but rather a history of Russian physicians as an occupational group. It deals with physicians who worked in the public sector of Russian medicine, about three-fourths of the entire profession. Nancy Mandelker Frieden demonstrates that public service was the norm for most Russian physicians and that, after the reforms of the mid-1860s, their employment in *zemstvo* medicine was representative of the profession as a whole. Her interpretations are supported by original and imaginative use of archival materials, especially of the state service records of more than sixteen hundred physicians.

Unlike their counterparts in the West, Russian physicians were not members of an autonomous "free profession," for they began as state employees, members of an estate (*soslovie*) replete with service ranks, uniforms, low state salaries, onerous forensic duties, and an ethical code devised by the government. Even after the introduction of *zemstvo* medicine, a pioneering free rural health program, the medical profession's "social profile changed very little over time" (p. 205). As a group, physicians enjoyed neither financial security nor social prestige. Gentry social prejudices against medical careers and gentry control of the *zemstvo* boards that employed the medical "hirelings" combined with bureaucratic meddling by the government and with the suspicions, and sometimes open hostility, of illiterate peasant masses to make the rural doctor's lot not only unhappy but unsafe.

Measured against this background, the relatively slow process of medical professionalization between 1856 and 1891 seems much more impressive. The government, because it needed the scientific expertise and the social service of *zemstvo* physicians, did unwillingly grant certain privileges to the profession in planning public health strategies. More important to the process of professionalization were the establishment, respectively in 1880 and 1883, of the publication *The Physician (Vrach)* and of the Pirogov Society of Russian Physicians. As Frieden sees it, medical professionalization had been achieved by the time of the Fourth Pirogov Congress in 1891.

The remaining two sections of the book deal with the interests of Russia's public physicians in social reform (1892–1902) and in political change (1902–05). A catastrophic famine and cholera epidemic at the beginning of the 1890s convinced many physicians that they had to press for more autonomy in dealing with problems of public health. They became social reformers and campaigned unsuccessfully for the abolition of corporal punishment, physicians' control over hygiene education, and for a decisive role in famine relief. Social reformism gave way increasingly to political activism after 1902. Only when their petitions were ignored by the government and their professional activities were thwarted at every turn did the physicians as a group join in the liberation movement in 1905. According to Frieden the image of the physician-radical is misleading. Pragmatists by training and necessity, Russian physicians preferred to diagnose the defects of the system and to recommend "means to remedy, not bury the political system" (p. 13).

The book contains useful statistical tables, an excellent bibliography, and interesting illustrations and anecdotes drawn from the scholarly and popular literature of the period. It is a thoughtful and provocative study.

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JONATHAN FRANKEL. *Prophecy and Politics: Socialism, Nationalism, and the Russian Jews, 1862–1917*. New York: Cambridge University Press. 1981. Pp. xxii, 686. \$49.50.

This book, the distillation of over twenty years of meticulous research, bears favorable comparison with Franco Venturi's *Roots of Revolution* in scope, size, and detail. Jonathan Frankel's work will undoubtedly come to be regarded as the definitive political history of Russian Jews and left-wing nationalism in English or any other language.

The author begins his story with Moses Hess and Aron Liberman, who anticipated the minimalist and maximalist poles of Jewish socialism decades before the formation of the Bund and the Poalei-Zion party. Frankel traces the influence of their ideas on Russian Jewry and explains how these ideas changed and became institutionalized under the impact of the 1881–82 pogroms. He discusses the politics and ideologies of Bundism and Marxist Zionism in Russia to 1907, offering detailed analyses of Chaim Zhitlovsky, Nachman Syrkin, and Ber Borokhov along the way. And he follows the development of Jewish left-wing nationalism in Palestine to 1914 and in the U.S. to 1918.

The good things about this book contrast with its revisionist interpretation of several important historiographical points, which in my judgment is naive in some respects. The problem seems to stem from Frankel's failure to distinguish among three types of causes to which historical explanations may (and, in the ideal case, do) refer. A historical event may be explained as (1) a consequence of people's *motivations* (the reasons they themselves state or suggest for their beliefs and actions); (2) the outcome of the *structural constraints* within which people are embedded (the changing patterns of social relations that render more or less likely their holding certain attitudes and taking certain actions); and (3) the result of *precipitants* (proximate causes that connect motivations to structural constraints). Political historians typically focus upon motivations and precipitants, drawing our attention away from the structural constraints that so fascinate social historians and historical sociologists. I should think that any full historical explanation must give due consideration to all three types of causes. But in practice scholars in the two camps often talk past one another, and Frankel is no exception in this regard.

For example, the crystallization of a more nationalistic program among early Bundist intellectuals has traditionally been viewed as the product of a force from below: the creation of a mass movement among Jewish artisans. The intellectuals, many of whom were assimilated, lived in Vilna where, in the 1890s, the most rebellious segment of the working class was Jewish. Seeking to forge ties with an agent of historical change, the intellectuals began to work

among Jewish artisans, learn Yiddish, recruit yeshiva students as agitators, and so forth. In due course, it has been held, the creation of these social ties increased the probability that the intellectuals would become less assimilated and adopt a more Jewish-nationalistic viewpoint.

Contrast this with Frankel's contention that "politics rather than sociology determined ideology" (p. 178). Frankel convincingly puts the case that Jewish labor movements did *not* become more nationalistic where established socialist parties allowed Yiddish-speaking sections autonomy and where established parties were strong enough to impose their internationalist will on the Jews. Precisely opposite circumstances obtained in Russia in the 1890s—with the opposite result. This argument focuses on precipitants and certainly rounds out our knowledge of why the Bundists became more nationalistic. But Frankel believes that it does not just complement, but actually replaces, the older view, which of course draws our attention to structural constraints. He is mistaken, as a *Gedankenexperiment* such as those Max Weber used to like to perform immediately shows. Imagine the growing embeddedness of some assimilated Jewish intellectuals in, say, the ethnically mixed working class of the Caucasian oil fields. If that happened (and it did on occasion) the intellectuals were likely to become internationalistic Mensheviks, not increasingly nationalistic Bundists. Now imagine that the Russian Social Democratic Labor party was strong and tolerant of an autonomous Jewish section in the 1890s. Assimilated intellectuals becoming attached to Jewish workers would still have had to learn Yiddish, adapt to the culture of their followers, and so on. In other words, they would have become more particularistic in their outlook in any event. Which sort of cause was then more important? I think there is little doubt that the structural constraint was. Frankel thus illuminates the question of what precipitated Bundist intellectuals to become more nationalistic, but he has not improved on Lamartine's 1848 reasoning concerning the fundamental constraints on radical intellectuals' ideologies and actions ("Je suis leur chef. Il faut que je les suive!"); nor has he improved on our understanding of the fundamental constraints that rendered probable the Bundists' nationalistic turn.

The same kind of problem emerges when Frankel seeks to explain why Ber Borokhov suddenly abandoned voluntarism for determinism after 1905. Before that year Borokhov viewed Palestinian colonization as a preparatory job to be carried out by an elite vanguard of pioneers; after the 1905 revolution began he developed a theory purporting to show how Palestinian colonization by the Jewish masses was historically inevitable. Again, a force from below is usually held to be principally responsible: especially the actions of Jewish workers during the 1905 revolution prompted Poalei-Zionists to

create stronger ties to Jewish workers—in 1906 the party headquarters was moved from Poltava, which was virtually devoid of Jewish workers, to Vilna—and to recognize their potential historical significance. But Frankel demurs. Borokhov's volte-face was, he writes, "more a question of tactics than conviction" (p. 359) and may even have been "a sudden reevaluation analogous to a religious conversion" (p. 360). Perhaps so. But it is difficult to understand why Frankel thinks his analysis of Borokhov's *motivations*—although colorful, complex, and informative—has any bearing at all on the question of which structural forces constrained Borokhov's thinking. That, after all, is the level of explanation at which the traditional view is pitched, and Frankel has by no means superseded the latter.

The idea that settlement of new lands was more important than the creation of a wage-laboring class found acceptance in Palestine during the years 1909–14, and one might make much the same criticism of Frankel's analysis of that process. But although no useful purpose would be served by multiplying examples of how Frankel confuses levels of explanation in his otherwise useful book, it is worth stressing the general point that political and social historians will probably continue carping at each other's work in a quite unproductive way until the whole causality issue is better clarified. In the early 1960s Karl Popper wrote that "[E]very explanation, whether historical or otherwise, assumes a certain world . . . with certain regularities, and in so far as it does, it assumes a kind of background of causal law. But it is only the *theoretical* sciences, not the *historical* sciences, that investigate, formulate, and criticize these laws. The historian takes them . . . for granted" (*Cambridge Opinion*, 28, p. 21). Reading Frankel's book I could not help wondering whether much of social and political history can be reconciled until Popper's view of what historians do not do is falsified.

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GEORGE TOKMAKOFF. *P. A. Stolypin and the Third Duma: An Appraisal of the Three Major Issues*. Washington: University Press of America. 1981. Pp. xi, 246. Cloth \$21.50, paper \$11.00.

George Tokmakoff concerns himself with the debates in the Third Duma (dominated by P. A. Stolypin) on the agrarian question, the limitations on Finnish autonomy, and the creation of the "western *zemstvos*." He writes for the informed reader; hence his spare elaboration of background details. Tokmakoff examines the part played in these debates by deputies of all major political persuasions and particularly by the nobility (without making a

special case for the liberal nobility who were nurtured and matured as members of that class). And he concerns himself as well with the attitudes of the Finns and Poles toward questions in which they were directly involved.

A prime purpose of a study of this genre is to demonstrate the capabilities and limitations of a Russian parliamentary institution operating at the end of a six-century-long autocratic tradition and faced with a changing society. To assume that by limiting himself essentially to a study of the debates the author is not aware of the political activities and attitudes of forces and factors outside of the Duma—a charge frequently brought of late against Duma historians—is to presume much more than the sophistication and insights offered by these studies would indicate. To elaborate in great detail, for example, on the attitudes and actions of the nobility outside the Duma as these affected Russian society and political life is to write another book. Familiarity with the nature of Russian society must be a part of the equipment of any student of the Duma, since the society was reflected in the operation and debates that affected the Duma's organization, nature, and its very existence.

Tokmakoff is generally sympathetic to Stolypin's moderate conservatism. He emerges, along with the Duma, as central to the effort to adapt the archaic autocratic system to a changing society. The parliament was itself a mixture of the old and the new and it molded and was molded by a figure who, with all of his frailties (likewise of the old and the new), was one of the most significant Russian statesmen of this century; Stolypin was one of the few figures capable of creating a potential for fundamental, positive change in society.

This study clearly indicates that despite Stolypin's protestations to the effect that he was not a parliamentarian, he was deeply involved in perfecting and protecting the parliamentary system. He was aware of the Duma's shortcomings but insisted that it had wrought a radical change in Russia and was taking root in the Russian mind. And Stolypin saw the Duma as a key element in his reformist effort. Whatever his rationalization of his threat to resign in the face of the Council's rejection of his *zemstvo* bill, that action might indeed have set a basic parliamentary precedent, as even Soviet specialists have sensed. It is indeed ironic that the most powerful supporter of the parliament in the administration was isolated by the Duma's opposition to his cavalier manipulation of its own Fundamental Laws.

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DIANE KOENKER. *Moscow Workers and the 1917 Revolution*. (Studies of the Russian Institute, Columbia

University.) Princeton: Princeton University Press. 1981. Pp. xiv, 420. \$30.00.

The Russian Revolution is usually viewed from the top down in both Russia and the West, that is, as a coup d'état rather than as a mass movement. But in the last decade a new generation of scholars outside Russia have emancipated themselves from the elitist kremlinological approach and have begun to look at the Bolshevik seizure of power more as a response to a mass movement than a sudden inspiration on the part of Lenin. This latest generation of scholars, like Rabinowitch, Rosenberg, Wildman, and Suny, have had the benefit of research in Russian libraries and archives. Diane Koenker's book on the 1917 revolution in Moscow belongs to this latter category.

Koenker provides a wide-ranging and illuminating analysis of working-class movements in Russia's second city in 1917, demonstrating beyond a shadow of doubt that there was a massive involvement of workers in political activity in 1917 and a significant growth in their political consciousness and sophistication. The Bolsheviks did not pick up political power from the gutter. Using an impressive amount of statistical material and rare periodicals, though disappointingly little from the archives, Koenker portrays the rich diversity of the Moscow working class. Like many advocates of history from the bottom up, she tries and succeeds in demonstrating that crowd action was conscious and purposeful in 1917, not impulsive and unpredictable. The first two chapters give a valuable overview of the city itself in 1917 and of the condition of its workers.

Although Koenker's work is a truly significant contribution to the study of the Russian Revolution, it does have two major flaws. Like many histories from the bottom up, it tends to reduce people to statistical categories. There are no individuals in the book, and one misses the high drama, color, and humor of this highly dramatic event. People were risking their lives in a grand gesture, pursuing the vision of a better life. The second flaw grows out of the first. She gives too little space to the October seizure of power itself and almost none to the political personalities and labor leaders that take part. This could be more easily forgiven if there were a number of studies of those events in Moscow in English, but there are none. Even Melgunov's excellent account was omitted when his book, *The Bolshevik Seizure of Power*, was translated into English.

The limitations of the book grow out of its origin as a doctoral dissertation with a somewhat narrower focus. Even with these reservations one can only welcome this highly original and valuable addition to Western research on the Russian Revolution. Koenker is quite right. It is time to study the

workers' role in this working-class revolution, and she has shown the rest of us how it ought to be done.

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SHEILA FITZPATRICK. *Education and Social Mobility in the Soviet Union, 1921–1934*. (Soviet and East European Studies of the Russian Institute, Columbia University.) New York: Cambridge University Press. 1979. Pp. x, 355. \$32.50.

Ever since they acquired an educated class the rulers of Russia have had trouble with it. The tsarist regime imported European schools to supply itself with administrative and professional staffs to keep the masses toiling, in the name of "Holy Russia," on projects given priority by those in control of the army and police. As one of their privileges, graduates of state universities and institutes kept harboring delusions of an independent role for themselves and their ideas. After the kaiser's army wrecked the tsar's army (and its remnants joined the Red Army), the exiled portion of the educated class that had converted to reborn Marxism got its chance to organize the work details, now in the name of abstractions with broader, if shallower, emotional appeal. By the 1930's the new commanders of the police and army were ready to solve the lingering problem of an educated class with delusions of independence. For one brief shining moment the communists toyed with the idea of replacing schools with on-the-job training programs. The opportunity passed and "common sense" prevailed. Fifty years later the Soviet Union is as embedded in the concrete of formal schooling as its enemies.

Sheila Fitzpatrick's fluent account of how the communists, under Stalin's leadership, opted for the "common sense" solution over the "ideological" one focuses on the higher education component of the first five-year plan. Utilizing Soviet archives, Vera Dunham's work on Soviet fiction, and the Hough files on Soviet officials, she traces the trajectory of the *vydvizhentsy*, the "promoted ones," those who were at the right place at the right time with the proper social credentials to be sucked up by affirmative action, Stalin-style, onto the "fast track" that led to a ceremonial position on the top of Lenin's tomb. Rapid expansion of VUZy (specialized higher institutes), higher quotas for certified proletarians, and a narrowing down of the curriculum to remove the temptation of independent thought (with the Gulag as backup) has supplied Russia with a loyal if unimaginative educated staff for fifty years. Fitzpatrick's account of how the Russian intelligentsia's habit of independent thought was broken is authoritative and lucid. We can only hope that she applies her considerable talents to continuing the story into

the purges of the late 1930s and that she finds a more helpful concept than "common sense" for the Stalinist solution of the early 1930s.

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WERNER G. HAHN. *Postwar Soviet Politics: The Fall of Zhdanov and the Defeat of Moderation, 1946–53*. Ithaca: Cornell University Press. 1982. Pp. 243. \$22.50.

This study sets out to overturn the generally accepted view of the *Zhdanovshchina*, the bitter purge that racked the Soviet Union from the end of World War II to the death of Stalin, and came to be known by the name of Stalin's top deputy, Andrei Aleksandrovich Zhdanov. Werner G. Hahn contends that Zhdanov was not in that period an "extremist" but rather a "moderate," and ultimately even a victim of the defeat of "moderate elements in the Soviet political establishment" (p. 9). He believes that Stalin's use of Zhdanov in re-establishing "orthodoxy" in Soviet life after the "relaxations" of ideological rigor during the war was "more a function of political maneuvering than an expression of ideological principle" (p. 10), and that, in fact, Stalin subsequently "stripped Zhdanov and his supporters of power and backed the most extreme chauvinists and dogmatists in their attempts to purge the social and natural sciences of unorthodox thought" (p. 10). Zhdanov is said to have been motivated largely by his need to defeat Georgii Maksimilianovich Malenkov's (and Stalin's?) desire to replace him as the dictator's possible successor.

In attempting to prove this thesis, Hahn relies largely on an analysis of struggles for power and preferment within the Communist party apparatus over a four-year period, 1946–49. He devotes four of the six chapters in his book (one to each of those years) to demonstrating that the contest led not only to Zhdanov's defeat, but also, after Zhdanov's death on August 31, 1948, to the purge of his "protégés" in 1949.

The remainder of Hahn's book, on the period immediately following Zhdanov's death, is devoted to showing that Stalin's policies grew increasingly brutal and "dogmatic," though "much in the 1950–53 period remains obscure . . . since Stalin's plans for a new purge were aborted by his death and he had carefully hidden his intentions and many of his actions from the outside world and even from his closest colleagues" (p. 14).

Although Hahn bases his thesis on "newly published decrees, obituaries, biographies and memoirs" (p. 13) as well as the standard newspapers and journals, he is, like many another, forced to extrapolate motives and intentions that cannot be proved. His main device is to connect the rise and fall of individuals in the intraparty struggles with their

presumed ideological "mentors," and in so doing ostensibly defines not only the conflicts but the sides individuals took on particular issues. Like everyone else, however, Hahn has considerable difficulty in obtaining "reliable biographic and organizational information on postwar Soviet leadership" (p. 14), and although he admits this himself, he nonetheless goes on to treat many of his assumptions as though they were fact. Such is clear from the language to which those who, without access to the files or the people involved, are forced when trying to disentangle the Kremlin web of intrigue; all those conditional tenses and waffling words Hahn is addicted to—"may," "might," "should have," "ought," "must have," "it is likely," "it seems," "it appeared to be," "some observers have felt," "all available evidence suggests," and so on.

Hahn is also trapped in the inevitable difficulties of defining terms in the Soviet political spectrum. What is a "liberal" or "conservative," a "moderate" or "dogmatist" or "reactionary" in that period? At an earlier stage, in the 1920s and 1930s, it was possible to give such terms more precise definitions than it has been since. Yet, no matter how vaguely defined, is it ever possible to see Andrei Zhdanov as a moderate? After all, the man had so often been assigned to do Stalin's dirty work—at the First Congress of Soviet Writers in 1934, later replacing Kirov as Stalin's man in Leningrad in 1939 after the Great Purges, and acting as the ruler of Leningrad and one of the ruling Politburo's nine members during the "Great Patriotic War." Hahn essays this by drawing attention away from Zhdanov's repressive role in literature and the arts.

What happened during the *Zhdanovshchina* is still not clear, nor are the reasons why it occurred. The government documents have never been released, nor are they likely to be made accessible even to Soviet historians, much less to foreigners. Zhdanov's private papers have never seen print, nor have his public speeches been collected and published. Most of the individuals involved are long dead and consequently beyond interviewing. Most important, we know so little of Stalin's mind, madness, and Machiavellianism at that stage of his life, of his paranoid's divide-and-conquer relations with his chief lieutenants, most of whom he had murdered, that it is virtually impossible to assign responsibility to individuals who were publicly identified with particular Soviet policies or events. Until such documents are available, if they ever are, we shall have to wait for a definitive work on the *Zhdanovshchina*.

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I. E. ZELENIN. *Obshchestvenno-politicheskaia zhizn' sovetskoi derevni, 1946–1958 gg.: Sostav i deiatel'nost'*

obshchestvennykh organizatsii, Sovetov, rost politicheskoi aktivnosti sel'skikh truzhenikov [The Sociopolitical Life of the Soviet Village, 1946–58: The Composition and Activity of Social Organizations and Soviets, and the Growth of Political Activity by the Village Toilers]. Moscow: Nauka. 1978. Pp. 244. 1 r. 40 k.

This publication of the Institute of History of the USSR Academy of Sciences is mainly a self-congratulatory piece for party activists; others must seek diligently within its pages to find a few morsels of useful information. But they are there, nevertheless. I. E. Zelenin reveals the extent of peasant alienation from Soviet rule in the German-occupied western parts of the Soviet Union during World War II. Citing archival data, he discloses that partisan bands infected with "bourgeois-nationalism" were active in the western Ukraine until 1949; there were 914 such bands in Belorussia in 1946. "Terrorists" in these areas prevented many people from subscribing to state loans after the war. The author admits that at the end of 1950 only 3 percent of all *kolkhozy* had a party organization in western Belorussia; 18 percent in the western Ukraine; and 20 percent in Estonia. He provides no statistics concerning *Komsomol* membership in these areas prior to 1953. The party had to send workers from eastern Belorussia and the eastern Ukraine to lead the recollectivization campaign that began in 1949.

Those who fancy Soviet statistics on rural party membership may be interested in the fairly complete data for Georgia, Azerbaijan, Kazakhstan, Uzbekistan, Tadzhikistan, Turkmenistan, and Kirgizia for 1946–58.

The level of this scholarship represents a decline from that of twenty years ago, during the "thaw," when self-criticism was in style in the USSR. Let us hope for better days ahead.

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NEAR EAST

NIKKI R. KEDDIE. *Roots of Revolution: An Interpretive History of Modern Iran*. With a section by YANN RICHARD. New Haven: Yale University Press. 1981. Pp. xii, 321. Cloth \$30.00, paper \$5.95.

It is impossible adequately to review within the confines of short notice a book that surveys the whole sweep of Iranian history from the beginning of the nineteenth century and has in addition an introductory chapter on "Religion and Society to 1800," which discusses some of the continuing themes of Iranian culture. Since Nikki R. Keddie devotes most space to her account of the Pahlavi

period (1921–79), I shall concentrate on this aspect of the book.

Keddie is, of course, right to stress the religious roots of the Islamic revolution of 1979 and to assert that the strength of Khomeini's Islamic revolutionary movement was underestimated by the Iranian government. One had hoped, however, that Keddie, as a historian, would have been able to take a more balanced view of the Pahlavi period and to show a greater recognition of the immensity of the problems facing the Pahlavi shahs at the same time that she points out their undoubted mistakes. Some important factors in the equation are either absent altogether or are understated to the point of distortion. Some of these may be inferred from Keddie's own analysis; others have to be supplied from one's own knowledge of recent Iranian history and of the Iranian psyche.

For example, the part played in the history of twentieth-century Iran by Russia, and since 1918 by the Iranian Communist party (both in its earlier manifestation as the Persian Communist party and in its subsequent manifestation as the Tudeh party), is as far as possible ignored by Keddie. The fact that Iran has had a stronger Communist party than any other Middle Eastern country might have been mentioned as one of the major problems with which the Pahlavis had to contend. When Communists are mentioned at all, one is led to believe that they were and are a negligible force. Because the Tudeh party was officially proscribed for much of the reign of Mohammad Reza Shah, Keddie implies that it was without political influence. Yet on May Day, 1951, members of the "banned" Tudeh party paraded through the streets of Tehran. After the purges of the Tudeh party between 1954 and 1957, the party's clandestine activities, and particularly its highly successful campaign to win the minds of Iranian university students studying abroad, were of the utmost significance. The Tudeh party may yet surprise many analysts of the Iranian scene, including the author of this book!

Another element that is not taken into account in Keddie's analysis of the situation is the Iranian character. Sadeq Hedayat's "masterpiece in the realistic-satirical genre," the novel *Hajji Agha*, is described as "an exposition of political, financial, personal and religious hypocrisy in which Hedayat presents in the person of his title character many of the main faults that have kept Iran from progressing soundly" (p. 197). The clue is there, but it is ignored.

Like so many opponents of the shah's regime, Keddie not only criticizes the Pahlavi shahs on matters where criticism is justified but denies them the luxury of a creditable motive for *anything* they did. If Reza Shah promulgated reforms, they were "mainly measures for centralization and efficiency"; he "had no interest in fundamental social reform to

help the popular classes" (p. 87). It is curious, then, that Reza Shah introduced a system of universal state education, showed keen interest in the emancipation of women, and laid the foundations of an industrial infrastructure that must surely have provided jobs for at least some members of the "popular classes." If Mohammad Reza Shah, after his accession in 1941, showed liberal tendencies, this could only have been the result of Allied pressure (p. 143). If he introduced women into the work force, it was because he thought it would "contribute to his modern image" (p. 179).

Paradoxically, the Qajar rulers are criticized for their *lack of* any centralizing "reform from above" (p. 272), and Keddie laments the fact that various reform-minded prime ministers during the nineteenth century were "dismissed before they could carry through their reform programs—largely because of the hostility of decentralizing or vested interests that would have been hurt by centralizing reform" (p. 274). Yet the problem of overcoming "decentralizing or vested interests" was apparently not a factor of importance in the twentieth century. Without any analysis of Iran's security needs, real or perceived, we are told that the late shah had a "virtual mania" for buying large amounts of military equipment (p. 176). He embarked on the path of economic planning, not as a consequence of social, political, and economic imperatives but because it was "fashionable" (p. 147). There was too much emphasis on "big showy projects" (p. 144); since we are not told what these were, we cannot judge, but if Keddie is referring to the large number of major dams, with resultant hydroelectric and irrigation benefits, which were completed during the 1960s and early 1970s, the emphasis was not, in my opinion, misplaced.

Major political developments between 1963 and 1977, we are told, "had none of the drama or importance of the early 1950s or early 1960s" (p. 178). Yet in the same breath the author refers briefly to the assassination in 1965 of the brilliant young Prime Minister Mansur by a disciple of Khomeini, and to another attempt on the life of the shah in the same year. These acts surely were dramatic, and since the murder of Mansur destroyed Iranians' last chance in the foreseeable future of moving toward a more broadly based participation in government, it was not without importance. It is strange, too, that Keddie does not consider the experiment with a one-party state from 1974 onwards, and the shah's attempts to involve intellectuals in the debate on the organizational structure of the Rastakhiz party, to be of importance. It was this period of alleged halcyon calm (disturbed only by an increasingly violent terrorist campaign against the government from 1971 onwards), that enabled Amir Abbas Hoveyda, appoint-

ed prime minister in 1965, to hold "the post for an extraordinary twelve and a half years, owing to the lack of economic and political crises in this period, the apparent success of the government's economic and social policies, and Hoveyda's ability to carry out the shah's wishes without appearing as a threat to the shah's power" (p. 179). Ignoring the offensive innuendo in the last part of this remark, the truth is that Hoveyda remained in office for an extraordinary length of time because he was an extraordinary man. It is more than regrettable that this is the only reference to his record in office.

The most useful chapter in the book, to my mind, is chapter 8: "Modern Iranian Political Thought," which is subdivided into "Intellectual and Literary Trends to 1960," by Keddie, and "Contemporary Shi'i Thought," by Yann Richard. The latter brings out clearly the striking inconsistencies, contradictions, and confusions in the ideology of "lay Shi'i political-religious thinkers" such as Bazargan, Bani-Sadr, and Shariati, who played such an important part in Khomeini's revolutionary Islamic movement. Shariati dreamed of an "Islamic totalitarianism" as the only political system that would respect completely the individual (p. 221). Khomeini's Islamic totalitarianism has not been noted for its respect of the rights of the individual. Shariati objected to democracy for the astonishing reason that it ensured "the crushing of the progressive minority by the conservative majority"—an objective that has been achieved in Iran today by Khomeini's fundamentally antidemocratic regime.

In her conclusion, Keddie notes that "as of March 1981, what is notable is not the unity or realism of Iranians, but growing discontent and disunity" (p. 271)—and, one might add, continuing unreality. Perhaps in time she will come to admit that discontent, disunity, and unreality are constants of Iranian political life. If so, she may even be willing to modify some of the harsh judgments on the Pahlavi shahs that she has made in this book.

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AFRICA

HERVÉ BLEUCHOT. *Les libéraux français au Maroc, 1947–1955*. (Études Historiques, number 2.) Aix-en-Provence: Éditions de l'Université de Provence; distributed by Jeanne Laffitte, Marseille. 1973. Pp. 282.

In the past decade a number of excellent works have appeared concerning Morocco and the French colonial presence there. Edmund Burke's *Prelude to Protectorate in Morocco* (1976), Kenneth Brown's *Peo-*

ple of Salé (1976), and Ross Dunn's *Resistance in the Desert* (1977) have focused on Moroccans' reactions to France and the imperial system. Other works have explored the colonial system or the Morocco of independence and national sovereignty. Few works have taken an in-depth look at those French who rose above colonizer status and supported the national aspiration of Moroccans. Hervé Bleuchot has written an excellent and informative volume dealing with those Europeans known as "the Seventy-Five," a group of French persons residing mainly in Morocco, who were outspoken as far as Moroccan independence was concerned. In one volume, complete with supporting documents, Bleuchot shows that the French attitudes toward Morocco and Moroccans varied from the cooperation of Lyautey with the sultan to the Berber Dahir of 1930, from one extreme of colonialism to the other, from a relationship of cooperation to repression. Bleuchot's thesis then becomes one that can be applied to many colonial situations: the colonial system was by its nature irrational, but the emergence of the "liberal" helped return the system to some semblance of sanity, for the "liberal" recognized the ultimate failure of the system.

Bleuchot asks the question, Was Lyautey a "liberal" as far as the colonial system was concerned? and responds, Yes, Lyautey was. Lyautey's liberalism, according to the author, was one of degree. He did not destroy the ruling elites or Moroccan culture. On the contrary, he encouraged them. *Colons* had less land than in Tunisia, and certainly when one compared the French in Morocco to the pampered *colons* of Algeria there were great differences. The discriminatory Berber Dahir of 1930, passed long after Lyautey's departure from Morocco, set the tone for another type of imperial control that lasted until after World War II. After the war, which saw a destitute France wage many colonial conflicts, an attempt to impose an outworn system on Morocco gave rise to Moroccan resistance and the Seventy-Five who dared to point out that colonialism in Morocco was indeed a tattered banner that should be at last furled and retired.

While the author presents an authoritative text on the liberals within Morocco, he includes also some very important documents that illustrate the awakening of the conscience of those men and women. For example, the original manifesto of the Seventy-Five is included, plus other relative documents. Taken as a whole, *Les libéraux français au Maroc* is more than just an informative work. It fills a definite need in Moroccan and North African history. Bleuchot defines a "liberal" only in the context of anticolonialism and support for decolonization, thereby avoiding useless and in this case meaningless debate over a person's commitments in other philosophical and political areas, and he only studies

those "liberals" in Morocco. Historians dealing with France, French colonial, African, and North African history should welcome this work as a major addition in that it deals with Frenchmen within a colony who supported and worked for an end to the colonial system.

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JEREMY WHITE. *Central Administration in Nigeria, 1914-1948: The Problem of Polarity*. Foreword by ADEBAYO ADEDEJI. Dublin: Irish Academic Press or Frank Cass, London; distributed by Biblio Distribution Center, Totowa, N. J. 1981. Pp. 369. \$35.00.

A crucial factor in the success of any federation is the number and relative strength of its component units. Too few units assure that conflict at the central level will always include the same protagonists. Too many units destroy the balance of power between center and periphery, and a federation with one or two units that are more powerful than the rest will experience conflict and instability due to the insecurity of the weaker members and the ambitions of the stronger. In retrospect, it can be seen that, when Nigeria became independent in 1960, its stability was undermined not only by the cultural diversity and relative economic standing of its ethnic groups but also by the structure of the federation itself. It had been launched on independence with three states: the Northern, Eastern, and Western, with the Northern being in area and population more than twice the size of the other two. With the secession of Biafra—the former Eastern state—in 1966, Nigeria was plunged into a bloody civil war. Since then it has experienced military rule and has experimented with reorganizing its federation on the basis of more states. By 1979, Nigeria had once more become a democratic polity, this time with nineteen states. It is significant that ten of these states are in the old North.

If it is agreed that the unbalanced structure of the Nigerian federation at independence was a major cause of the subsequent instability and civil war, the question arises: Who or what was responsible? In so many words, Jeremy White, who is a senior lecturer in history at the University of Lagos, makes the same assumption and asks the same question. His thesis is that, beginning with Lugard himself, the responsibility lies mainly with the British colonial administration.

Although the main thesis is arguable, for the Nigerian specialist this book is fascinating. White does go over some of the same territory first traversed by Coleman, Crowder, Ezera, Awa, and Sklar, but he brings in new information and new insights, and he marshals these to make a point. One

is reminded again, but perhaps more forcefully than before, how the colonial administration in the upper North came to identify with the political fortunes of the emirs and the native authorities. This made it especially difficult for those few spokesmen who wanted to divide up the old North in order to make Nigeria more viable.

One can agree with White's premise that the British favored the regal North over the parvenu South without necessarily accepting his conclusion. There may have been other factors that made for Nigeria's being malformed at independence. Among these may have been the relative power of the British administration with respect to the emirs and the relative responsibility of Nigerians themselves.

To argue as White does one must assume that the British administration in the upper North had a free hand in carving that territory up into more than one region or state, and yet this assumption is questionable. It is clear from White's own description that indirect rule rested heavily on the goodwill and cooperation of the emirs themselves. When that cooperation momentarily came into question, as during the Satiru rebellion, so did the whole structure of indirect rule. It may be that it was not sympathy for the emirs or lack of vision for the future alone that prevented the British from carving up the North. It may have been a very real fear that tampering with the powers of the emirs and with indirect rule itself would bring the structure of British colonial administration crashing down and not only in the North.

The author is quite right that by the time of the Willink commission of 1957, it was too late for the British alone to do very much about the structure of Nigerian regionalism. Nigerians, on the other hand, did have another chance. In the federal elections of 1959, none of the three major nationalist parties, including the Northern People's Congress, had a majority of the 315 seats. It turned out, fortuitously perhaps, that any two of the regionally based parties could have created a ruling coalition. It is significant, therefore, that when Azikiwe of the National Council of Nigeria and the Cameroons (NCNC) had an opportunity to form an all-southern coalition with Awolowo's Action Group, thereby excluding the North from power at the center, he demurred. One of his reasons was that he feared Northern secession.

If under the British the emirs may have had the power to destroy indirect rule, under the nationalists they had the power to destroy Nigerian unity. This power—or its reputation—was enough to prevent reform. It is significant that it was only when the federal government had demonstrated its overwhelming military force during the Biafran war that

the North, in conjunction with the other regions, allowed itself to be divided.

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DUNSTAN M. WAI. *The African-Arab Conflict in the Sudan*. New York: Africana. 1981. Pp. xxvi, 234.

This book is a valuable contribution to the Southern Sudanese quest for identification as a discrete political and regional group within the Sudan in distinction to the plethora of tribal and ethnic peoples inhabiting the Upper Nile. The author briefly summarizes the well-known and obvious ethnic, cultural, racial, and religious differences between the Northern and Southern Sudanese that, of course, are the fundamental bases for the African-Arab conflict in the Sudan. The British attempted to deal with this division by the implementation of a "Southern Policy," designed to eradicate Northern influence in the Southern Sudan and to promote the indigenous African cultures. "Southern Policy" has been roundly castigated by Sudanese scholars as perpetuating disunity, but Dunstan M. Wai argues that in fact it was simply the recognition of existing conditions. He believes that it was a correct policy and that its abolition was "the greatest error of the British colonial administration," coming too late to safeguard the political position of the South in an independent Sudan (p. 51). Thus unprepared for self-government and independence, the South remained socially and economically deprived, its people were pressed to assimilate with Muslim Arab culture, and Southerners consequently sought to free themselves from that domination.

The author describes the Sudan as a praetorian state, a particularly apt description, because the Sudan lacked political institutions to moderate group political action so that power was wielded by those elites, the praetorian guards, who sought to seize power at the expense of others, among them the Southerners. They sought to achieve unity by the imposition of Islam and Arabic, all of which resulted in Southern resistance and seventeen years of civil war in the South. The justification for rebellion was not, however, simply Northern oppression but the indisputable fact that the Southerners "were not involved in the process of decolonization and they had not exercised their right to self-determination" (p. 183). Presumably that right of self-determination was ultimately exercised by their success on the field of battle, by which the Sudan government agreed to a negotiated settlement at Addis Ababa in March 1972 whereby Southern demands for recognition were agreed upon by an autonomous status within the independent Sudan.

Despite the fact that Southern leaders did participate in the negotiations and supported the agreement, which over the past ten years has clearly been accepted by the Southern populace, Wai still argues that the exercise of the right of self-determination has not been exhausted. It is an inalienable right, presumably to be applied at any time. Thus, he argues that the present autonomous status of the Southern region established by the Addis Ababa Agreement could be changed by the Southerners at any time. "It puts the Southern Sudan in a transition" (p. 192).

Wai's conclusion implies the Southern Sudanese have that inalienable right to choose a different destiny, and the whole thrust of his analysis of the African-Arab conflict in the Sudan appears to mean greater Southern independence from the North, perhaps even complete separation. The belief in the sanctity of the inalienable right of self-determination must result, however, in the acceptance of the results of that right. Ironically, the author invokes it to invest with legitimacy the determination by the Southerners to seek a more independent status, but the inalienable right of self-determination is being employed at this very moment not to demand greater independence for the Southern region but exactly the opposite—to redivide the South into tribal regions. If successful, such an exercise of self-determination would dissolve the Addis Ababa Agreement, Balkanize the South, and make each smaller region less politically powerful and thus even more, not less, at the mercy of the greater economic and political strength of the Northern regions—a result that the author would deplore as a retrogressive step in the search for Southern self-hood.

Thus, in providing a very useful analysis of the African-Arab conflict in the Sudan, Wai makes a valuable contribution in sorting out the participants in a complex struggle. The justification for regarding the present state of the Southern Sudan as transitional could, however, easily lead to precisely the opposite conclusions for which the author has made such a good case.

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RANDALL M. PACKARD. *Chiefship and Cosmology: An Historical Study of Political Competition*. (African Systems of Thought.) Bloomington: Indiana University Press. 1981. Pp. xii, 243. \$25.00.

Modestly subtitled "An Historical Study of Political Competition," *Chiefship and Cosmology* is a bold and largely successful attempt to reformulate conven-

tional views of the dynamics of African power. Few other works in print are likely to give greater respectability to the structuralist argument (à la Levi-Strauss) that political institutions have less to do with empirical reality than with the normative models through which such reality is perceived, explained, and acted upon. It is the crux of Randall M. Packard's thesis that African perceptions of chiefly authority, including ideas about its nature, functions, and ritual legitimacy, constitute the single most important mechanism by which the institution of chiefship among the Bashu retained its historic continuity in the midst of environmental and societal changes.

Chiefship and Cosmology is the product of extensive fieldwork among the Bashu of eastern Zaire, supplemented by archival research in Belgium and Zaire and a solid grasp of the anthropological and historical literature on the interlacustrine zone. The result is a fascinating account of the process of state formation at the microlevel through the manipulation of ritual and symbolic resources. The author shows how during the nineteenth century the introduction of new resources (trade and firearms), which posed new threats to the incumbent, did not so much erode traditional views of chiefship as strengthen their hold on the collective consciousness of the Bashu; how certain ritually important groups continued to exercise influence (especially when drought and famine were seen as "caused" by a breakdown of ritual control); and how, under colonial rule, the administrative role that the Belgian-appointed grand chief was expected to play on behalf of his people came into conflict with Bashu values and "created a prolonged struggle between Belgian administrators and Bashu leaders" (p. 169).

Packard's book provides a crucially important corrective to those interpretations of state formation in precolonial Africa that stress the application of naked military power, the invocation of "divine kingship," or the astuteness of alliances and coalition formation. The selection of allies, no less than the application of force or the use of symbols, cannot be dissociated from the cultural codes and cognitive criteria through which choices are made. This is where Packard's analysis makes a vitally important contribution to our understanding of the structuring of power relationships in traditional Africa.

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ANN BECK. *Medicine, Tradition, and Development in Kenya and Tanzania, 1920–1970*. Waltham, Mass.: Crossroads Press. 1981. Pp. viii, 114.

Ann Beck has long been interested in problems of health care and administration in East African his-

tory. This short book, her third monograph on the region, is much more narrowly focused than the title suggests. Beck's concern is with health planning, both under the British and under the radically different political philosophies of independent Kenya and Tanzania.

Concern for rural health developed in the 1920s, but the British were slow to implement dispensary schemes, partly for lack of funds and staff and partly for fear of poor quality services. Plans of the 1950s gave greater stress to rural and preventive medicine, and this trend has been continued and elaborated upon by the independent states. Indeed, one is struck by the continuity of thought since the 1950s and by the apparent similarity of goals of Kenyan and Tanzanian planners.

An interesting chapter sketches attitudes toward "traditional" healers. Contrary to some recent assertions, colonial officials and physicians in East Africa made no attempt to suppress indigenous practices or practitioners. In the last decade or so, Kenyan and Tanzanian health administrators, like some of their contemporaries elsewhere in Africa, have sought ways to enhance collaboration between traditional and scientific medicine.

The book has little about what actually happened to health care delivery, except to note the continuing financial and cultural problems that have made it impossible to implement any plan fully. They have all failed. This reader had the impression of hopping from study commission to study commission, from report to report, with little sense of what, if anything, resulted from often commendable projects and plans. Beck's style is sometimes fuzzy and her comparisons and conclusions are not explicit. She does not give full credit to local research efforts or to interest in nutrition during the colonial period. Her study does raise some basic health administration issues, suggesting that policy changes occurred slowly in both colonies/countries and that fiscal and other constraints are still powerful. Despite the rhetoric of independence, African socialism, and the Alma Ata conference and despite some real progress, continuity seems more important than change.

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ADELE SMITH SIMMONS. *Modern Mauritius: The Politics of Decolonization*. Bloomington: Indiana University Press. 1982. Pp. xii, 242. \$22.50.

Adele Smith Simmons has given us an excellent book on the twentieth-century history of Mauritius. In its two hundred pages she captures the diversity and complexity of this island nation and describes

the country's progress from colony to independence within the Commonwealth.

Simmons uses a chronological approach to the subject with appropriate pauses for topics such as the development of elites within the different ethnic elements of the population. As she shows, Mauritius is a small, remote island; its nearly one million inhabitants are all descended from immigrants and include South Asian Hindus and Muslims, Afro-French creoles, overseas Chinese, and French. The British, notwithstanding one hundred fifty years of colonial rule, are an infinitesimal minority. No single community has a simple majority, and, as Simmons points out, this is at once a curse and a blessing. It is menacing in that it has encouraged politics to evolve from a class basis ("sugar barons" versus the laboring classes) to a largely communal basis. Every politician in Mauritius today denounces "communal politics"—the appeal to ethnoreligious groups that pits one against the other—but each practices it as best he can by codewords, becks, and nods! On the other hand, heightened inter-communal suspicions serve also to produce blocking minority coalitions, which effectively prevent any single community (for example, the Hindus) from altering the democratic, parliamentary structure of the constitution. It may well be that, as in Switzerland's history, suspicious and at best grudgingly tolerant communities can provide the foundation for a stable democratic country.

Mauritius is not Switzerland, however. Simmons stresses the severe economic problems facing the island. Apart from a brief idyllic period of high sugar prices, which fortunately coincided with the first years of independence, the one-crop sugar economy is in trouble. The basic compromise by which the Franco-Mauritian sugar barons grudgingly relinquished political pre-eminence to the predominantly Hindu Labour party in return for political stability and a still largely unfettered control of the economy is touched on by the author, if not as bluntly as here stated. The growing role of tourism in this island, a Southern Hemisphere equivalent of Puerto Rico and Hawaii, is also noted, together with the indifferent success of attempts to develop light industry.

Modern Mauritius is old-fashioned political history in its best modern incarnation with due weight given to economic, cultural, and sociological development. Simmons takes us through the intricacies of Colonial Office policy during five decades, showing us how its innate conservatism coexisted happily for a hundred years with the dominant and extremely conservative French planters. The story of the gentle pressure from Whitehall that led the new Labour party gradually to responsible government, in both senses of the word, and without irretrievably alienating either the planters or other ethnic communal

groups is fascinating reading. It was a thankless task but one in which both "Little Englanders" and Kiplingesque imperialists could take pride, even if the grumbling along the way from almost all Mauritian groups and the pressure of postindependence events make it likely that few Mauritians will ever express gratitude.

My criticisms of this book are minimal indeed. Perhaps the importance of Diego Garcia as *Mauritius irridentus* is slightly overstressed, and the recent development of self-sufficiency in potato culture might have been noted as providing a little relief on the food-import front. But I emphasize that this is a unique book, clearly written, well organized, and of interest also to the nonspecialist. There is nothing else like it in the field.

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ASIA AND THE EAST

JOSEPH NEEDHAM. *Science in Traditional China: A Comparative Perspective*. Cambridge: Harvard University Press or Chinese University Press, Hong Kong, 1981. Pp. x, 134. \$12.50.

First presented as the Ch'ien Mu Lectures at New Asia College, Chinese University of Hongkong, the five chapters of *Science in Traditional China* now enable a greater audience to share in the erudition, wit, and comparative perspectives of a grand master. Compared often with Gibbon, John Stuart Mill, Marx, and Darwin, Joseph Needham is in his fifth decade of dedicated inquiry into the Chinese scientific tradition. Today, his lifework *Science and Civilization in China* is eleven tomes complete, with eight or nine yet to come.

Already established in biochemistry when he "discovered" China, Joseph Needham's remarkable quest began with these questions: Why did modern natural science fail to develop in China? How was it that a group of young Chinese scientists he met in the 1930s in England thought in much the same way as he did? In the introduction Needham acknowledges the few who influenced his thought and the many who aided in his work. Here he stresses also social and economic factors operating in Chinese and European engineering, biological and medical, and astronomical sciences. Four fascinating chapters with illustrative diagrams follow: "The Epic of Gunpowder and Firearms, Developing from Alchemy," "Comparative Macrobiotics," "The History and Rationale of Acupuncture and Moxibustion," and "Attitudes toward Time and Change as Compared with Europe."

Needham establishes the differences between

Chinese and European science, and indeed the Chinese anticipation of many modern fields, in the Chinese *philosophia parennis* of an organic materialism (or organicism—the interrelatedness of all phenomena). Therefore, a good portion of the emphasis has been given to Taoist alchemy, which pointed the way not only to the invention of gunpowder and fire, but also, through the search for macrobiotic elixirs, to the rise of modern medical chemistry. Acupuncture and moxibustion are accounted for in the current contexts of neurochemistry and immunology. Chinese attitudes toward time and change are masterfully compared with Indic, Hellenic, and Judaeo-Christian ideas and shown to be remarkably constant in face of the innovativeness of science. Through all, Needham accepts the great role that social and economic structures play in the goals and results of East-West science. Therefore, early Chinese skills in iron casting were absorbed into customary usage and threatened no social structure. In the West, however, such skills produced the cannon that toppled the castles of feudalism and forged the transformative machines of the Industrial Revolution.

This brief book is extraordinary in many senses of the word. From philosophies to social structures, from arts and crafts to military strategies, Needham weaves a historical web of *realia* as informed by the Indian, Hellenistic, Arabic, and European traditions for an understanding of Chinese science. In approach, scope, and subject matter, Needham's work compares and contrasts civilizations and world views more than is usual in traditional history of science. Some may argue, as some have argued in the past, against viewing civilization through the monocle of science. Very few, if any, can escape being extended by the range and dimensions Needham has given science and civilization. Light and yet profound, incisive and yet discursive, this small volume takes the reader on a pleasurable excursion in the organicism of Needham's universe of interrelated phenomena.

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W. E. CHEONG. *Mandarins and Merchants: Jardine Matheson & Co., A China Agency of the Early Nineteenth Century*. (Scandinavian Institute of Asian Studies, Monograph Series, number 26.) London: Curzon Press. 1979. Pp. xii, 298.

COLIN N. CRISSWELL. *The Taipans: Hong Kong's Merchant Princes*. New York: Oxford University Press. 1981. Pp. x, 249. \$29.95.

The turbulent development of China's foreign commerce was of major international consequence in

the eighteenth century, influencing the organization and financing of the British empire, especially India, and that of the Spanish New World, much of whose gold and silver flowed to China. In the early decades of the nineteenth century, this China trade was shaken by several shocks—the rise of the market for Indian opium in China, the collapse of the Spanish empire, the decline of the British East India Company, and the rise of the great private trading companies such as Jardine Matheson and Company. The two books under review, which are nearly opposite in style and approach, both deal with the new era wrought by these private traders. Although both are flawed, they do make contributions to our understanding of the important commercial relations that formed the backdrop for China's often violent relationship with the West during the nineteenth century.

Colin N. Crisswell's *The Taipans* focuses on the great private businessmen (the taipans) who operated out of Hong Kong. Crisswell begins with a discussion of the conditions in the early part of the nineteenth century and continues in chronological fashion until World War I. The most interesting portion of the text, however, is that which deals with the rise and prosperity of the large opium houses, Jardine Matheson and Company and its rival, Dent's. Matheson led the fight to end the British East India Company's control of the China trade and pressed for the aggressive policy that led to the Opium War (1839–42). By the 1860s, nonetheless, the heyday of these great opium houses was eclipsed by the anti-opium movement in England, the development of native opium in China, and the spectacular rise of the Sassoons, a Jewish merchant family from Baghdad via Bombay who gained control of the opium supply market in India. By 1867 Dent's had folded and a few years later Jardine moved out of the opium trade into general commerce and finance. Crisswell completes his discussion with coverage of the rise of Butterfield and Swire and Sir Paul Chater. The table of contents lists an epilogue that might have discussed later events, but this section was not contained in the copy of the book that I examined.

This is a work of rather slight scholarship. Although the author, who lives and works in Hong Kong, has apparently consulted some early letters of the Jardine family and a few contemporary periodicals, the bulk of the book seems based on readily available, English-language secondary materials. It is difficult to identify his sources with precision for there are few footnotes and the bibliography is quite brief. Crisswell renders his material in an old-fashioned narrative, with limited analysis of larger social and economic issues and with a strong hint of nostalgia for the period. Chinese perspectives are noticeably absent. The author deals with the Canton

city question and the origins of the Arrow War but nowhere refers to either Frederic Wakeman's *Strangers at the Gate* or J. Y. Wong's biography of Yeh Ming-ch'en, both standard works on this topic. The author discusses the taipans' disappointment at the slow growth of Anglo-Chinese trade in the decades following the Opium War, yet he makes only one slight reference in another context to the Taiping Rebellion. That conflagration ravaged central China for years, left twenty to thirty million Chinese dead, and surely influenced China's economy and trade.

Does this book, therefore, have nothing to recommend it? On the contrary, minimal as the scholarly contributions are, the book is written in a lively and engaging style. The author details the interesting lifestyle of the taipans—the brothels, the churches, the racial and social snobberies, and the amusements at Happy Valley race track. The taipans rushed to make great fortunes in opium trade and then invested their lucre in respectable property in England, buying status and acceptability for their families. Some, like the Jardines, chose a quiet upper-class life, while others, like the Sassoons, ostentatiously flaunted their wealth and conspicuously socialized with the Prince of Wales, later Edward VIII. Crisswell's narrative account is filled with lively and amusing stories of these individuals. Bearing in mind the work's limitations, many readers will find it useful.

W. E. Cheong's *Mandarins and Merchants* is in total contrast to *The Taipans*. It is solidly researched, painstakingly footnoted, and based on archival materials, particularly the London archives of Jardine Matheson and Company. Although there are background chapters on the early nineteenth century, this study is more narrowly focused than Crisswell's and centers on the decade of the 1830s, in which the British East India Company lost its monopoly of the China trade, the powerful Jardine Matheson and Company was formed, and serious problems developed in the East Asian commercial world. Of particular concern to the author are details of the financial aspects of the trade, including credit and monetary transfer arrangements, cost factors, and price fluctuations.

Unfortunately, this work is almost unreadable and will be of use only to specialists. The author overwhelms the reader with exhaustive detail on trading conditions, but he offers almost no analytical framework or even summary passages to indicate what points the voluminous factual information was meant to illustrate. There is no real discussion of personalities, very little on the broader questions involved in the trade (the opium problem in China is considered only as a factor influencing the market prices, for instance), and not even a clear institutional history of Jardine itself. Cheong has divided his

discussion of commerce in the 1830s into chapters organized by commodity (opium, cotton, silk, tea, and indigo). As a result the chronology of his discussion is completely lost. The very brief, eight-page conclusion is disappointing, for the reader had hoped by that point to have some overall interpretative framework. In sum, while Cheong is to be praised for his exhaustive spadework in the Jardine archives, his study will benefit only a select few.

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JOHN H. FINCHER. *Chinese Democracy: The Self-Government Movement in Local, Provincial, and National Politics, 1905–1914*. New York: St. Martin's Press. 1982. Pp. 276. \$27.50.

John H. Fincher wants us to know that China has experienced democracy, and not just since the "Democracy Wall" of 1978 or the elections of 1980. China mobilized an electorate of two million in 1909 and forty million in 1912, and "these two elections produced assemblies whose democratic achievements are a matter of record" (preface). He argues on structural grounds that Chinese democracy belongs to world history: in the Netherlands, England, and France, representative government emerged as the complement of national power; the same was true in China (though more briefly). Therefore, democracy should be viewed as "a universal category rather than a cultural trait" (p. 22). But Fincher never defines democracy. He uses the term "democrat" promiscuously for Chinese of unspecified status or function who are associated with some form of self-government, for example: "early Chinese democrats" (p. 31), that is, since the Sung dynasty; "local democrats" (p. 60); "elected democrats" (p. 227); "Nationalist democrats" (p. 233); or "militant democrats" (p. 241). He never quotes these "democrats" nor asks whether they espoused democracy as an end in itself or simply as a vehicle to national power. His failure to conceptualize "Chinese democracy" leaves unsupported his assertions that it belongs in the same field of discourse with that of the West.

The real strength of the book is described by its subtitle. Its three-tiered analysis achieves a perspective on China's revolutionary process rare in recent scholarship. The intricacy of the discussion and the variety of its insights cannot be briefly conveyed. Fincher shows how the structure of the Chinese polity as it evolved after the eighteenth-century reforms made the provinces the main arenas for the political transformation of the last Qing decade. The gentry-dominated provincial assemblies contended with provincial officials and the new central

ministries by building "their horizontal links with each other and, via the National Assembly, to Beijing" (p. 183) rather than functioning in the vertical structure envisioned by the Qing reforms. Meanwhile, local-level counties were "left behind" under the magistrates and the ineffectual local councils, which were unable to cope in any ordered way with "the wide range of tasks set for them, as heirs to a great number of traditional local institutions abolished since 1900" (p. 232). The self-government movement thus "transcended the traditional local concerns of the gentry" (p. 244), and its failure to integrate the counties into the new order left them a prey to militarization early in the republic.

Fincher concludes that the Qing state in 1911 was far more stable than is conventionally believed: "policies designed to conserve the centre in fact succeeded up to that point when their success at the periphery began to undermine their success at the centre" (p. 219). Far from collapsing in chaos, the state was perpetuated through some of its reformed ministries and important municipal governments, including Beijing. We miss this broader perspective, Fincher observes, by too often seeing China's twentieth-century history as "a collection of disconnected provinces," or as "the rise of Chinese nationalism" in the hands of a few parties or leaders" (p. 220), a tendency recent scholarship has done little to correct.

It is regrettable that this important book is not more readable and better produced. Originally dictated onto tape, the text is sprinkled with directive phrases ("as noted above," "as already implied," "as we shall see presently") that fail to overcome redundancies and turgid syntax. It includes neither bibliography, glossary, nor index.

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RICHARD CURT KRAUS. *Class Conflict in Chinese Socialism*. (Studies of the East Asian Institute, Columbia University.) New York: Columbia University Press. 1981. Pp. x, 243. \$22.50.

As Henry Kissinger has pointed out on the basis of personal experience, Mao Zedong was a supreme realist in foreign affairs. In domestic affairs, on the other hand, Mao is a probably unique case of a leader who grew more radical as he grew older—or to be more exact, after a serious illness at the end of 1953—until he turned the political system he had done so much to create more or less upside down in a "great upheaval" that he called the Cultural Revolution. This he did in the name of equality, an ideal in which he deeply believed—at least for others, since he avoided manual labor and liked to live well.

Equality resembles a level body of water: in the pure case, nothing moves. This seems to be recognized by orthodox Marxism-Leninism, which holds that under "socialism" everyone is to be rewarded "according to his work" and thereby implicitly admits that inequality is at least partly the result of differing individual endowments and not merely of exploitation by some ruling class. One of the respects in which Mao was not an orthodox (that is Soviet-style) Marxist-Leninist was in his passionate insistence on prolonging "class struggle" aimed at equality into the stage when "socialist construction" ought, again according to orthodox Marxism-Leninism, to have been the order of the day instead.

The book under review is an able, carefully researched, and well-documented exercise in political sociology. It deals mainly with Mao Zedong's use, or abuse, of the classical Marxist-Leninist concept of class struggle in the era of socialist construction to rationalize his campaign against the emerging Chinese "new class," the party and state bureaucracies. This campaign was probably even more central to Mao's thought and political strategy than was the concept of class struggle, inherited of course from the Marxist tradition. Although Richard Curt Kraus notes that Mao's important speech of April 1956, "On the Ten Great [or Major] Relationships," contains no class terminology and was made at a time when class struggle was not really on the Chinese Communist party's agenda, he does not mention that the speech proposes a two-thirds cut in the party and state bureaucracies, a goal that was not to be approached until the Cultural Revolution a decade later.

Kraus carries the story into the post-Mao period, when his successors have accused Mao of having stressed class struggle long after he should have shifted the emphasis to socialist construction. But Mao, like his immediate successor Hua Guofeng, has been posthumously charged merely with "mistakes." The Gang of Four, with whom Kraus like many others assumes Mao to have been in close sympathy, have been convicted of "crimes." Other leftists, who after the fall of the Gang of Four proclaimed support of "whatever" Mao was supposed (by them) to have said, have been quietly purged.

In spite of these victories for the cause of pragmatism and socialist construction, which currently flies the banner of the Four Modernizations and is symbolized and led by Deng Xiaoping, Kraus is clearly correct in believing that Maoism and resistance, especially among the military, to growing inequality and the virtual suspension of class struggle promoted by the present leadership are not dead. Indeed, the well-known ferment associated with modernization may very well produce in the China of the future an increase in social and political

turmoil. If so, it would not be surprising, in view of the pervasive character of the Maoist tradition, if that turmoil wore the label of class struggle.

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EARL H. KINMOUTH. *The Self-Made Man in Meiji Japanese Thought: From Samurai to Salary Man*. Berkeley and Los Angeles: University of California Press. 1981. Pp. xi, 385. \$28.50.

This is a much needed piece of research. Despite the superabundance of generalizations about the Japanese work ethic, there are few careful studies of how success and ambition have been conceptualized in Japan and fewer yet that attempt to understand this in the crucial decades of the Meiji period (1867–1912), when the Japanese version of industrial society first emerged. Earl H. Kinmouth has given us an excellent essay in historical semantics aimed at discovering "modal interpretations" for the concept of success for Meiji youth. His main sources are the popular youth-oriented magazines of the day. Often aptly titled as *Talent Forum* or *The Wheelbarrow of Knowledge* or even *Battlefield of Student Writing Brushes*, these inexpensive periodicals carried inspirational features, advice columns, didactic fiction, and, sometimes, essays submitted by the young readers themselves.

Kinmouth has painstakingly mined this extensive if largely ignored material in search of links between youthful ambition and the concepts of wealth, honor, patriotism, filial piety, character, diligence, and personality. Perhaps the most important of his findings is that what was perceived as constituting success and the royal path to it were by no means constant over these five decades. In the initial wild excitement of the years immediately following the overthrow of the Tokugawa shogun, the enormous social demand for new types of skills spawned a tremendous range of opportunities and a heady optimism that would seem poignantly naive, if it were not for the large number of documented cases of unpredictable leaps into the front ranks of the new elite. During the 1880s and early 1890s, however, there came the realization that such shortcuts to honor and wealth were being systematically closed off as the new social order stabilized. Advancement now lay through the patient application of effort in the classrooms and the examination halls of the newly established educational system. The "dynamics of bureaucratic capitalism" were beginning to dictate that youth be prepared for limited roles in large organizations, and this placed a premium on an "other-directed" or "personality" ethic rather than any attempt at overwhelming the competition through superior performance (pp. 273–74). Only a

tiny number of Meiji youth turned to social activism in protest, although sufficient numbers did flirt with the twin sirens of nihilism and hedonism to engender a flurry of concern about "anguished youth." Kinmouth finds the "material origin of anguish" as well as the docile acceptance in the "shift in terms of trade for educated youth" (p. 212). Thus the new preoccupation with literary and philosophical interests among the nonconformist segment of Japanese youth in the late Meiji is seen as "part of a quest for alternatives to conventional *rishin* [advancement] or compensation for its loss" (p. 227). Some may find this treatment of how the new climate of opinion took shape a bit mechanistic, but Kinmouth presents the single most useful summary in English or Japanese of the existing data on such matters as the increasing difficulty of gaining entrance into elite schools and the decreasing economic returns of education when measured by the declining market value of the university degree.

Kinmouth is not content, however, with his careful analysis of this change in social values. He also speculates on its wider significance for subsequent Japanese history, arguing in the conclusion that this shift led to an enormous pressure for conformity on recruits into the Japanese elite, and this pressure "dwarfed" Japanese leaders in the interwar period, ultimately producing the failure in leadership that brought about the Pacific war. This is an intriguing thesis, but one that is far too large for the carefully researched monograph we have here. It may well be that the pressures for conformity were greater in a late developing Japan than in earlier developing industrial societies and that leadership elsewhere in the 1930s was less "dwarfed" than in Tokyo. But if Kinmouth is to argue this successfully, he will need to produce a second book. Whatever the subject of his future writing, the evidence is clear from this book that we shall benefit from reading it.

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BEN-AMI SHILLONY. *Politics and Culture in Wartime Japan*. New York: Clarendon Press of Oxford University Press. 1981. Pp. xii, 238. \$29.95.

Japanese society and politics during World War II suffer from guilt by association with Japan's European Axis allies. Ben-Ami Shillony, however, accentuates the differences between wartime Japan and totalitarian or fascist regimes. His aim is to revise that "distorted image of a docile nation manipulated by a few fanatics in uniform" (p. vii). Drawing on an extensive variety of published, Japanese-language primary sources and on secondary literature, Shillony convincingly demonstrates that Japan was

different from its Axis partners in its adherence to the rule of law, in its stable political institutions, and in its respect for the weight of precedent. These features stamp Japan as unique among the Axis powers, and Shillony concludes that in wartime Japan even "oppression was relatively mild" (p. 13). It remains debatable, however, exactly why Japan was different.

According to Shillony, Western attitudes had deeply modified traditional Japanese concepts, and this modification created a cultural dilemma during the Pacific war. Although the West was the mortal enemy, the Japanese could not discard Western-style modernization and still hope to win the war. It appears that in this synthesis of Japanese and Western concepts, however, the former remained the touchstone. The political downfall of wartime premier General Tojo Hideki illustrates this point.

The success of Tojo's government depended on its ability to cooperate with other Japanese elites—the imperial court, the bureaucracy, the diet, and the navy. Its daily operation relied on practices such as consensus decision making, compromise, and emphasis on group loyalties. The author stresses such values throughout his narrative and identifies them as the qualities that influenced Japan's response to total war. Even as powerful a figure as Tojo was unable to control arbitrarily the values that shaped the interaction of politics and society. Yet those characteristics that Shillony uses to emphasize Japan's differences appear to be those esteemed as peculiarly Japanese in nature. Western modifications to this cultural force of habit are not as strikingly apparent.

The Japanese press, for instance, showed great durability, and this, in turn, attested to the stability of the Japanese state. Did such stability derive from traditional Japanese values, recently incorporated Western ones, or a combination of the two? The answer frames one's perspective of why wartime Japan was different. Shillony argues for a combination of values, but the examples he adduces seem to emphasize the dominant role that traditional Japanese beliefs and attitudes played in wartime Japan. Westernization is like the Japanese approach to antisemitism, a love-hate affair with the West, to be sure, but an affair with only a superficial gloss of Western culture. This may suggest that Japanese values provided a natural immunity to the virulent European "isms" of the 1930s and 1940s.

Shillony skillfully presents in a succinct and polished style a great amount of information heretofore unavailable to the English-language reader. In his brief concluding chapter, he challenges both traditional Japanese and Western historiography about Japan's wartime experience. Further speculation on the wealth of material he surveyed would have produced a more stimulating conclusion. Nev-

ertheless, by showing us what wartime Japan was not, this slender volume should encourage historians to investigate further that little-understood period.

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ROBERT REPETTO *et al.* *Economic Development, Population Policy, and Demographic Transition in the Republic of Korea*. (Studies in the Modernization of the Republic of Korea, 1945–1975; Harvard East Asian Monographs, number 93.) Cambridge: Council on East Asian Studies, Harvard University; distributed by Harvard University Press, Cambridge. 1981. Pp. xxii, 294. \$15.00.

Population pressures have often been given as a significant cause of war in world history, and the right to breathing space, to Lebensraum, has often been given as one justification for invading neighbors. Now that large-scale peaceful or aggressive international migration has become a policy difficult to contemplate, rapid population growth is more commonly, but not universally, seen as a major obstacle to the internal development of a country and a threat to its social and political stability. This is why the demographic transition from traditional conditions of high fertility and high mortality to the desired modern situation of low fertility and low mortality can be considered the most important phase in the demographic history of a country and an integral part of its modernization. Unfortunately, it has proven difficult to study systematically the interaction of the demographic with the socioeconomic transformation of a country, and few comprehensive studies are available.

Because of its rapid development in every domain since the early 1960s, Korea constitutes a remarkable case to study. In particular, it has experienced one of the fastest fertility transitions ever recorded, with a decline of 40 percent in the crude birth rate between 1960 and 1975; only the city-states of Hong Kong and Singapore have done better. The striking Korean case raises many interesting questions regarding the relative impact on fertility of factors such as: recent wars; social fluidity in a relatively unstratified society; a great degree of equality in the distribution of physical wealth as well as education; the deliberate choice of an export-led, labor-intensive economic growth strategy; the rapid pace of urbanization and industrialization; the entrance of a large number of women in the labor force; and the rapid spread of education together with the increasing cost of raising children. There are three other factors not mentioned in this book: very high population densities, great cultural homogeneity, and political unity going back twelve centuries.

This book, which is one of a collection of eight volumes studying the modernization of Korea during the last twenty-five years, is only partially successful in its objectives of explaining what has happened in Korea and how that country's experience clarifies our understanding of the demographic transition. This volume is an excellent source of information for patient readers willing to follow up leads wherever they may take them. But it remains a collection of six individual papers by authors with heterogeneous backgrounds, analytical approaches, interests, writing skills, and objectives. To be sure, all the questions that have been raised are addressed and receive at least partial answers; but the results are not fully consolidated when this particular subject demands integration. Each paper in isolation has merit, but the whole in this case seems to be less than the sum of the parts.

The introduction by Robert Repetto presents the aims of the book and reviews the contribution of each author. It is somewhat awkward in the sense that Repetto does not discuss each paper in the order followed in the book; but what is the right order? The second chapter by Tai Hwan Kwon is particularly important, because it brings the required long-term perspective to the understanding of the demographic transition. It is only a short but useful summary of the author's work at Seoul National University's Population Center. The third, and longest, chapter by Dai-Young Kim and John E. Sloboda constitutes a very good critical review of the literature on Korean migration. It evaluates the relationship between mobility, economic development, and urbanization. The link between migration and the demographic transition, however, is only a relatively limited element of this chapter.

The fourth chapter by Repetto presents a multivariate analysis of the recent fertility decline and its possible causes. It constitutes an interesting effort to develop a formal quantitative model of the demographic transition, more accurately of the fertility decline since 1960. Except for reasons of computational convenience, however, it is difficult to understand the use of a recursive model that implies unidirectional causal relations for an analysis of what appear to be strongly interactive forces.

The last two chapters by Song-Un Kim and Peter Donaldson deal with population policies in Korea. I preferred the chapter on the Korean family-planning system in Korea by Donaldson to the review of policies by Kim; the latter often seems to be more a rationalization of changes after the facts than a description of actual policies.

In spite of its unevenness, this is a useful book that is extremely rich in references on Korea, but it is not the last word on the subject. There is still ample room for a Korean scholar to treat the same subject with a greater integration of cultural, social, political, and historical factors with quantitative

analyses. Of the eight volumes of this Harvard-Korea project, I still find the study of Korean entrepreneurship by Leroy Jones and Il Sakong the most original and the most rewarding to read. By focusing on the Korean entrepreneur they greatly enriched our understanding of the formulation and implementation of Korean economic policies. Could not a focus on the Korean family produce similar results for our understanding of the demographic transition?

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NEMAI SADHAN BOSE. *Racism, Struggle for Equality, and Indian Nationalism*. Calcutta: Firma KLM; distributed by University of Southern California, Los Angeles. 1981. Pp. xix, 275. Rs. 80.

Ellis, the cockney manager of a timber firm in George Orwell's novel, *Burmese Days*, says, "Here we are, supposed to be governing a set of damn black swine who've been slaves since the beginning of history, and instead of ruling them in the only way they understand, we go and treat them as equals." If none have better portrayed British contempt for the populations of India than Orwell, no one has better documented such racial and cultural prejudice than Nemaï Sadhan Bose. The author covers principally the period from the administration of Lord Cornwallis beginning in 1786 to the founding of the Indian National Congress in 1885. The emphasis is on Bengal where controversy was most intense. Comparatively little attention is given to other areas.

Bose attributes much of British arrogance to the attitudes of Christian missionaries. In the eighteenth century, East India Company policy opposed interference with Hindu or Muslim religion or social institutions. Proselytizing was forbidden within company territories. It was agitation by William Wilberforce, Charles Grant, and others that eventually led to the Charter Act of 1813, opening India to such activities. The contempt of many missionaries for Indian religions and customs encouraged Europeans to consider themselves morally and intellectually superior.

Ironically, English education at missionary schools in Bengal provided Indians with the first real opportunities to protest against discrimination. Physical abuses of natives were, in practice, not subject to judicial remedies, and efforts to make Europeans subject to local courts were successfully opposed by them as "Black Acts." The exclusion of most Indians from the civil service similarly heightened conflict and agitation for equality under law. During Viceroy Ripon's administration, the historic Ilbert Bill provided that judges and magistrates were to have limited powers to try British subjects.

The long agitation for and against culminated in the passage of an amended act so emasculated as to be meaningless.

The present study is not the first attempt to assess the role of racism in British-Indian relations. Kenneth Ballhatchet published *Race, Sex, and Class under the Raj* (1979), and Bradford Spangenberg edited *British Attitude toward the Employment of Indians in Civil Service* (1977), but Bose has done the most thorough and insightful analysis. He has ably researched primary sources in the India Office. He has examined Hansard, the files of vernacular newspapers, the letters of Macaulay and other principals, missionary tracts, and the records of Indians and British residents active in the nineteenth-century controversies.

If one might quarrel at all with Bose's emphasis on racism, it would be to suggest that despite so many instances of sheer racial intolerance, the overriding factor was probably the larger consideration of cultural prejudice. The author contends that it is useless to distinguish between the two. Nevertheless, it is abundantly clear that many British leaders—including the Reverend James Long, Macaulay, John Stuart Mill, and other influential Utilitarians—hoped to bring Indian subjects into a British brotherhood that would transcend race. They were appalled at the spectacle of a tiny minority of Europeans denying justice to a native population outnumbering them by more than two thousand to one. What English leaders could not stomach was a culture that they considered barbarous, immoral, and superstitious. Relatively few, such as Max Müller and Sir William Jones, could appreciate the richness of Indian civilization.

On the Indian side, the charismatic Bal Gangadhar Tilak (mentioned but once in Bose's final chapter) led the revolt in Maharashtra with an appeal for *swaraj*, not on the issues of racial discrimination so much as on those of self-determination. He rebelled against domination by what he considered "alien" cultures, whether Muslim or European.

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EDWARD INGRAM. *Commitment to Empire: Prophecies of the Great Game in Asia, 1797–1800*. New York: Clarendon Press of Oxford University Press. 1981. Pp. xvi, 431. \$49.50.

In the preface to his useful study, *The Beginning of the Great Game in Asia, 1828–1834* (1979), Edward Ingram dismissed his colleagues at Simon Fraser University for caring nothing about diplomatic history. With this new study he claims to have found one friend to help, but splendid isolation is often the lot of those who toil in the same vineyard. Still,

Ingram, as an expert player of the "great game," presses on, filling in part of the background to his earlier work with the present volume, *Commitment to Empire*.

Ingram assumes, rightly, that anyone interested in the "great game," the larger strategy of the Napoleonic wars, or the role of India in British policy must read this book, but the resulting self-assurance is exasperating at times. For every brilliant insight provided, and there are several concerning the relationship of empire to foreign policy, the reader must suffer numerous repetitions. To cite only the most glaring example, Wellesley's determination to create an Anglo-Indian empire is expressed no fewer than forty-seven times in various forms. Indeed, the book appears to be a conglomerate of Ingram's many articles on the subject over the last decade; judicious editing would have much improved the work as a whole.

For all that, the main arguments are sound. That Napoleon's Egyptian adventure was the real beginning of the "great game," or at least its "prophecy," is perhaps less startling than Ingram would have us believe, but no one has yet explained why in such detail. To some, there was advantage in holding a French army in Egypt and thus away from Continental battles; to others, responsible for India's security, the French were a serious threat to empire. To Wellesley, ambitious for personal power, the French provided a golden opportunity to destroy the internal Indian balance of power and in its place build his own empire.

For the school believing in the threat to India, the issue was how to stop Napoleon. Sea power could not drive the French from Egypt, though it might keep them there. But who would provide the needed troops? Turks, Persians, and Afghans were all considered, and, though in the end Britain had to do the job itself, the debate marked the overture to the main acts of the nineteenth-century "great game." In Ingram's view, Britain was shifting from a Mahanian reliance upon sea power to a Mackinderish focus on the Indian glacis. The two geopoliticians had yet to formulate their theories, but Ingram has put them to good use.

It is in consideration of "balance" and "empire" that Ingram functions best. In the course of the search, however, individuals become chess pieces with specific purposes and functions, some more powerful, some less, but all carved on rather simple lines. Despite Ingram's mastery of the records they left behind, they all lack dimension: Wellesley's empire, Dundas's defense of India, Grenville's balance of power—only Pitt is human enough to dither and fumble, but Pitt is hardly mentioned. If other scholars disagree with his interpretation, they have simply misread the documents or otherwise erred, as have J. S. Galbraith (p. 116), G. J. Alder (p. 164),

H. Furber (p. 124), C. N. Parkinson (pp. 296, 336), and others. It is a contentious style of history, rather in the mode of Ingram's mentor, Elie Kedourie, but that, as Kipling might agree, is the way of the "great game" and will only make the book more interesting to its devotees.

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STANLEY WOLPERT. *Roots of Confrontation in South Asia: Afghanistan, Pakistan, India, and the Superpowers*. New York: Oxford University Press. 1982. Pp. viii, 222. \$14.95.

This volume by Stanley Wolpert is uneven. Some portions are of high caliber and command admiration; other portions are rather superficial or tread very familiar ground. It is, moreover, not entirely clear what the major goals of the book are. Wolpert's objective appears to be to illuminate the background to the current confrontation over Afghanistan, which was brought to a head by the Soviet military incursion since late 1979. Wolpert also gives us a summary history of South Asia along with a discussion of Hindu religion and society as well as of Islamic society. The book includes his interpretation of the Anglo-Russian "rivalry" in the Afghan area during the nineteenth century as well as an evaluation of U.S. policy and behavior toward South Asia during the past thirty years. Finally, he offers his prescription for future U.S. policy toward South Asia.

Intertwined through much of the text is a muted nostalgia for those bygone days when Great Britain maintained "law and order" throughout South Asia and "protected" it from the voracious and unappeased appetite of the Russian "bear" in Central Asia. There is also an undercurrent of negative assessment of the character and intentions of the government of India, especially under the leadership of Indira Gandhi.

By far the best parts of the book are those that evaluate and criticize U.S. policy toward South Asia during the past thirty years. As Wolpert makes quite clear, our policy and actions toward South Asia have been uninformed, frequently counterproductive, and generally muddled. We have spent billions on arms and related assistance for Pakistan. The net effect has been to destabilize the region as well as to provide the weapons with which the Pakistan military oligarchy has fought India and suppressed internal discontent. Our policy toward India has confused all concerned and has alienated many of our friends there. I have not read a better critique of our South Asia performance than that which Wolpert provides.

His cursory treatment of the South Asian past

adds nothing new to general knowledge, and the discussion of Hindu society is sketchy and unsympathetic. His discussion of Islam posits a unity and a "brotherhood" that is belied all too clearly by the record of seven hundred years of wars between Muslim principalities and rulers, including the recent massacre of countless thousands of Muslims in East Pakistan by the West Pakistan army.

The discussion of the nineteenth-century "great game" between Britain and Russia has problems with motivation and causation. Other published academic sources handle the matter more effectively.

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JAMES FRANCIS WARREN. *The Sulu Zone, 1768–1898: The Dynamics of External Trade, Slavery, and Ethnicity in the Transformation of a Southeast Asian Maritime State*. Singapore: Singapore University Press. 1981. Pp. xxvi, 390. Cloth \$40.00, paper \$28.00.

This book is divided into three parts: the first, and longest, part deals with the expansion of international and internal trade carried on by natives, Chinese, and Europeans and the impetus this gave to the emergence of the Sulu sultanate; the second part is about slave raids and trade; and the third looks at slavery in the Sulu political economy and society.

The importance of trade in the rise of the Sulu state between 1772 and 1876 is not a new phenomenon in the history of indigenous maritime states of Southeast Asia. Trade here, however, must be seen in perspective: low-value, small-volume, but frequent and extensive in network as befits a rural, maritime, and dispersed empire. But this was only the ostensible livelihood of the state and its peoples and only one strand in the weave of the region's political economy. Slave raids and trade were the complementary economy that provided other important threads in the economic fabric. Their existence, in James Francis Warren's view, was endogenous to trade and politics of that state, and slavery and the ethnic subgroups that thrived on this dark side of its economy were essential ingredients in the make-up of the society. The evidence adduced is persuasive of the coterminous existence of a legitimate state and society going about its business alongside an outlaw way of life and of making a living. The distinction between the two, it would appear, is otiose, as the one was as much a part of the state and society as the other. It is also conceded that, in the latter, the heterogeneous ethnic subgroups found a homogenous mode of existence and a *raison d'être* within the Tausug state.

We are, however, not enlightened as to whether

this dark underside of the economy and the activities associated with it were peculiar to this period in Sulu history. Equally perplexing is why these ethnic subgroups were dislodged from their original communities to become a subject people and an appendage in the Tausug state. A change of political status, loss of independence, changes in lifestyle and occupation (which, in the case of the Maranaos were drastic), and physical migration en masse require more than the brief explanations given. The attractions of Sulu prosperity is a good but perhaps insufficient reason; a volcanic eruption is by no means conclusive; the legitimizing of hereditary titles would only come after the migration; and "social unrest in their homeland" is not given much weight by the author. Could this last have been more serious and, as most of these groups were from Mindanao, could Spanish policies after all have been more disruptive than the author has indicated?

Curiously, piracy and smuggling failed to find a place in the volume even though European records are replete with them. It is difficult to see the pursuit of slaves obliterating all interest in ships and cargoes captured at sea and movable property and chattels captured during raids on coastal settlements. Giving slave trade, raids, and slavery a natural place in the Sulu ethos does not make other clandestine and predatory activities just go away.

This enterprising volume is strongly recommended for its original contributions in parts 2 and 3, which together give an interesting view of Sulu society in a period of prosperity. The survey of trade in the first part is also useful. The work is carefully researched and documented and the bibliography extensive.

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M. C. RICKLEFS. *A History of Modern Indonesia c. 1300 to the Present*. Bloomington: Indiana University Press. 1981. Pp. xii, 335. \$22.50.

Though specialized monographs abound, historical surveys of Indonesia in English are few and far between. Since the histories of B. H. Vlekke (rev. ed., 1959), J. D. Legge (1964), and Bernard Dahm (1971) (none of which is mentioned in M. C. Ricklefs's bibliography), there has been little to choose from. Ricklefs describes his work as "a textbook designed for the serious student," which aims to provide "a basic but detailed narrative" of Indonesia's history since the advent of Islam. This book, however, provides little more than a barely dressed up chronology of events, names, and dates. Major controversy surrounds the important turning points of Indonesian history, and one would have

thought that even an introductory text would benefit from some interpretation, analysis, or indication of the scope of alternative points of view. These are all but absent here. Perhaps this is because, as Ricklefs indicates, he has discovered that students, though considering interpretation "more readable," also find it "less useful" than detailed description. The result is that Ricklefs's book has become a dry as dust compendium, offering an encyclopedic, highly compressed coverage of the major phases of modern Indonesian history. That interpretation and narrative detail can be held in balance even in textbooks (which are thus more likely to stimulate students to further inquiry) apparently has escaped the author, who is a professor of history at Australia's Monash University.

Highly conventional in its historiographic structure and approach, Ricklefs's book traces Indonesian (particularly Java's) history from the first historical evidences of the adoption of Islam in the eleventh century to the present era of President Suharto's *Orde Baru* ("New Order") in twenty short, tersely written chapters. Synoptic brevity again and again provides for only the most cursory treatment of areas other than political history. There is a "superficial survey" chapter of some precolonial Indonesian literary and artistic achievements. But of the internal feudal structure or economic base of empires like Majapahat or Mataram, we glimpse as little as of the deepening social and cultural interrelationship between colonial Dutch, Eurasians, and various Indonesian groups. Yet, in a six-page chapter concerned with the Indonesian states from 1300 to 1500, it is deemed necessary to devote half of a page to naming in detail all the exports such as woodresins, honey, or nutmeg, from Malacca, Java, Timor, and other areas!

The Dutch colonial historiographic distinction between Java and the "Outer Islands" also appears again and again in these pages, and the result is that there is little perspective on developments in and perceptions of Indonesians located on the periphery of colonial power. Thus, the "history of modern Indonesia" once more is treated as if it were primarily the history of Java. The trailblazing work of G. J. Resink (and of his critics!), which would have provided a much needed correction of this traditional and distorted perspective, is not even mentioned in the bibliography or in the notes and references section. Particularly deficient is the treatment of perhaps the most important single historic turning point of the past generation, the failed coup attempt of 1965. Ricklefs warns against "interpretations" that seek to explain the coup "solely" as a result of the machinations of the Communists, the army, Sukarno, or Suharto. This reviewer is unfamiliar with any interpretation of the coup assigning such exclusive responsibility. The problem, rather, has

been in determining the relative importance of the role played by each participant group or figure. But again one would not know this either from Ricklefs's text or from his highly selective and limited bibliographic references.

Given all such deficiencies one hesitates to recommend this book even as a text. For the specialist it has even less to offer.

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C. M. H. CLARK. *A History of Australia*. Volume 5, *The People Make Laws, 1888-1915*. Melbourne: Melbourne University Press; distributed by ISBS, Bearerton, Oreg. 1981. Pp. xv, 448. \$40.00.

This is volume 5 in C. M. H. Clark's series on Australian history, and the period it embraces marked a time of enormous change, which spans the years from the emergence of political labor parties in the various colonies through the federation of those colonies into the Commonwealth of Australia and ends in the middle of the First World War.

It is becoming fashionable in certain Australian circles to denigrate the considerable achievement these five volumes represent, and the advent of this most recent volume aroused a chorus of snarling and abusive commentary. Perhaps it is an example of that Australian propensity to cut down the tall poppies and to ridicule those ideas and questions we prefer not to confront directly. Whatever the cause, the upshot has been to direct attention away from this account of Australian development between 1888 and 1915 and to center it on the personality of the author. This is a great pity because in volume 5 Clark demonstrates that he can still tell a fascinating story extraordinarily well.

The range of sources he consulted is immense and the author's control over a variety of difficult-to-handle materials is nothing short of masterful. Clark possesses a strong sense of identification with Australia, and his judgments are firmly stated reflections of such an outlook. This can be demonstrated in his enunciation of the characteristics of George Turner, an Australian variant of the British liberal and the colonial representative of an Anglo-Australian consciousness that fought for paramountcy with Australian nationalism and for the hearts and minds of colonial Australians during these years. Turner is described as "the quintessence of the average man" who had done nothing to improve the uninteresting appearance nature had given him. He was also a "model bourgeois" with a passion for detail and for hard work that normally characterizes men of limited horizons and that went hand in glove with

Turner's *raison d'être* of balancing the books. "Turner was British to the backbone in his devotion to practical good sense. He was also British to the backbone in glorying in his lack of political doctrine, in viewing all displays of emotion with disapproval, sexual passion with disdain, and compromise as the supreme political genius of the English-speaking peoples of the world" (p. 122).

By way of contrast the reader detects a measure of approbation for the sentiments espoused by radical colonial laborites whose journal the *Tocsin* in 1901 described the visiting heir to the British throne, the duke of Cornwall and York, and his wife as "two strong able-bodied and healthy state loafers," as "professional drones," and as the "figure-heads of a banal social clique" (p. 195). The lickspittle behavior of Australian social climbers when in the presence of this pair was roundly denounced as anti-Australian and as one of the grotesqueries of colonial "groveling."

This volume traces the rise to power of the Anglo-Australians and the political failure of the radical nationalists who despised the compromises and caution of those who persisted in the search for a new Britannia in the Pacific, whatever the cost in blood or self-respect. The book concludes on the eve of that great confrontation between the Anglo-Australians and the Australian-Hibernian nationalists over the issue of conscription for overseas military service, a confrontation destined to split the Australian community and to fracture and endanger for decades to come the flimsy consensus on which the Commonwealth of Australia rested at the end of 1915.

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P. F. DONOVAN. *A Land Full of Possibilities: A History of South Australia's Northern Territory*. New York: University of Queensland Press. 1981. Pp. xxii, 267. \$24.50.

Explorers by their exaggerations and delusions have frequently contributed to the surge of European enterprise into little-known areas. Verney Lovett Cameron's description of the vast wealth of the African interior stimulated the cupidity of Leopold II and others, which led to the "scramble." Likewise, John McDouall Stuart, the renowned explorer who crossed Australia from south to north in 1862, reported to the capitalists of Adelaide that the northern coast was ideally suited to European settlement and that it contained some of the most fertile land he had ever seen. His testimony excited great enthusiasm and led the South Australian government to ask Britain to give them control over the

"Northern Territory." Their request was granted in 1863. South Australia retained jurisdiction until 1911, when the government finally concluded that the territory was a "white elephant" and presented it to the Commonwealth of Australia. The story of a pursuit of a chimera for almost fifty years is a remarkable example of the credulity of supposedly intelligent businessmen and politicians.

The dreary succession of failures between 1863 and 1911 had a variety of causes but of fundamental importance was the lack of understanding about the ecology, particularly of the northern coast, which was the primary attraction. South Australians had some acquaintance with the central region, which they knew was a harsh environment of low rainfall and little agricultural potential. But they had little understanding of the far north, an area of two seasons—very dry and very wet (an average of sixty inches of rain)—extremes with which they were unprepared to cope. The dreams of a north of family farms and small pastoral holdings were washed away, but new arrivals repeated the experiences of their predecessors.

There have been several works that deal with the failures of South Australians in their Northern Territory, but most have concentrated their attention on the politicians of Adelaide, to whose mismanagement they assign the primary responsibility for the dismal experiences in the north. P. F. Donovan convincingly demonstrates that this indictment overstates the responsibility of government. Although various governments were benighted, their follies did not deter financiers and speculators from pursuing ventures that were based on unrealistic hopes of success. They overestimated their technology and underestimated the problems they would encounter. The gold "rush" of the 1870s brought small miners into a territory where operations were difficult in the dry season and almost impossible in the wet. Transportation of stores was expensive, and the diggings were quartz rather than alluvial. The Chinese labor imported from Southeast Asia was able to cope with the problems better than its white counterparts, and during the thirty years before South Australia relinquished the territory, they were the majority of the nonaboriginal population. The relative success of the Chinese made South Australia ambivalent about restricting them in the Northern Territory, in contrast to the treatment of the Chinese in other parts of the continent.

The book contains a great deal of detail that will be of interest only to specialists, but it provides an interesting commentary on the costs of ignorance of the environment.

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UNITED STATES

THOMAS SOWELL. *Ethnic America: A History*. New York: Basic Books. 1981. Pp. 353. \$16.95.

When a scholar in another discipline sets out to change a paradigm that historians have largely agreed upon, his findings are not likely to be received with an excess of sympathy. This has been the experience of Thomas Sowell, an economist who is at once adventurous and conservative. The contributions his writings make toward an alternative formulation of American ethnic history are less easily noticed than are his errors and ideological predilections.

Ethnic America is a comparative study of the development of nine ethnic groups over time: Irish, Germans, Jews, Italians, Chinese, Japanese, blacks, Puerto Ricans, and Mexicans. Historians, while comparing the advancement of certain white ethnic groups, have generally been unwilling to compare the various races because the circumstances of their incorporation into American society were so different. Indeed, it is generally assumed that the history of blacks and the history of European immigrants are distinct and incommensurate. Sowell, however, treats all groups alike by posing for each the same question: in what ways and to what extent did it adopt the traditional American ethic of thrift, work, and education?

Stated so baldly, this approach seems extremely invidious, and many will find it so. The author's interest, however, is not in ranking the ethnic groups but in highlighting the fundamental importance of different cultural heritages, as opposed to immediate external circumstances, in shaping their destinies. For example, Mexican-Americans have gained particular advantages and incurred particular liabilities from their family system; blacks of different origins—antebellum “free people of color,” Southern slaves, and West Indians—have fared quite differently. Thus Sowell's attention to geographical and generational differences within ethnic groups helps to soften the invidious thrust of his argument, as does his recurring observation that all groups change and that those who still “lag” have nonetheless come a long way.

Ethnic America rests on a very wide range of secondary sources supplemented by census bureau statistics that Sowell has used in imaginative ways. (Critical readers will wish to refer frequently to the tables published separately in *Essays and Data on American Ethnic Groups*, edited by Thomas Sowell [1978].) So abundant are the data Sowell now gives us that his new book approximates a general history of the groups he has chosen. This eclectic, almost encyclopedic approach contributes to the upbeat spirit of the book. It also sacrifices rigor. Pursuing

his argument only episodically, Sowell sometimes dwells on immediate external circumstances when they suit his convictions, as in attributing the weakness of Puerto Rican organizations in the United States to the impact of the welfare state, while at other times underplaying those circumstances, as in making light of racial discrimination in the North before the great black migration from the South. I wished for more consistent rigor also in the use of statistics and of sometimes dubious secondary sources. There are too many groundless statements (that 42 percent of the Irish were still servants in 1890, for example) and too many questionable statements that are made with reckless assurance. Sowell rejects out of hand the elaborate survey data on American ethnic groups compiled and published in recent years by Andrew Greeley without demonstrating that his own very different census-based data are more trustworthy.

Although Sowell must be read cautiously, he has much to say that is instructive. Preoccupied with themes of oppression and resistance, historians have only begun to explore the varied responses of ethnic groups to one another. Unfortunately, a scrutiny of those responses in the light of traditional criteria of success does not yield a new paradigm but only revives and extends an old one in some surprising ways. Ethnic groups have not merely responded to America, they have also changed it. An inattention to those structural changes makes Sowell's work less historical than he intends it to be.

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BROOKE HINDLE. *Emulation and Invention*. (Anson G. Phelps Lectureship Series on Early American History.) New York: New York University Press. 1981. Pp. xix, 162. \$22.50.

In six fast-moving chapters Brooke Hindle advances the thesis that the creativity of graphic artists and inventors originates on common intellectual ground. What they share is an ability to think spatially about entire working systems or compositions. Such thought, Hindle argues, is essentially nonverbal and synthetic. In practice much depends on the ability of artist-inventors to emulate their peers and improve the state-of-the-art by bringing existing elements together in new combinations; hence the appropriateness of the book's title, *Emulation and Invention*.

Invention, Hindle maintains, is not so much a problem-solving activity in the received tradition of science as it is an extended act of insight, conceptualization, and implementation. Such work is synonymous with systems design and proceeds by degrees,

partly with “flashes of genius” but more often by fits and starts. At each stage of the creative process an inventor’s sense of proportion and spatial relationships remains critical. Just as an artist “paints over elements that fail to relate satisfactorily to other parts of the picture” (p. 54), so an inventor works in a synthetic manner with the mental manipulation of images and components to render an effective working design.

Hindle supports this thesis by examining the history of two “conceptually new” technologies in the nineteenth century: the steamboat and the telegraph. In both cases individuals trained in the visual arts and crafts played key inventive roles. With the steamboat the chief protagonists were John Fitch, James Rumsey, William Thornton, Benjamin H. Latrobe, and Robert Fulton. Whether executing landscape paintings, buildings, or steamboats, all of them exhibited an effective capacity for synthetic spatial thinking. The same observation applies to the invention of the telegraph, although its development centered mainly on the work of one person—Samuel F. B. Morse. In Hindle’s view, Morse’s “great strength remained a quality of mind that permitted him to manipulate mental images of three-dimensional telegraphic components as well as complete telegraph systems, altering them at will and projecting various possibilities for change and development” (p. 107). For Morse the convergence of art and invention is forcefully illustrated by the “canvass stretcher” receiver he devised for the telegraph around 1837. In fact, a great strength of the volume is the presence of excellent “pictorial essays” that follow Hindle’s chapters on the steamboat and the telegraph. By focusing on certain features of these new technologies and discussing them with reference to the visual artistry of their inventors, Hindle significantly enhances one’s understanding of spatial or nonverbal thinking.

While it remains to be seen how far the analogy between art and invention can be extended, Hindle’s analysis of these two important nineteenth-century technologies is persuasive. The book ends with a broad-ranging discussion of “The Contriving Mind” and why spatial modes of thinking flourished in America. Here Hindle points to a number of cultural and environmental differences that seemed to give the United States advantages over Europe. “The vast riches and the unexampled opportunity offered by the undeveloped continent,” a contagious “spirit of enterprise,” fewer “rigidities in commercial life,” and, above all, an “unquenchable enthusiasm” for labor-saving devices—all these factors encouraged a certain “drive and imagination” that propelled America to world leadership in the realm of invention (pp. 138–41). What is less clear from the analysis is whether popular enthusiasm for new technologies persisted once the action shifted from

the inventor’s shop to the practical world of industrial application. Hindle seems to suggest that it did. Yet recent studies of early industrial communities indicate that public reaction to new technologies was more mixed, depending on time, place, and circumstance. One leaves the volume with a cloudy understanding of the boundaries between invention and innovation. To what extent are they complementary processes? To what extent are they different? Do different stages of creativity elicit different social responses?

With *Emulation and Invention* Brooke Hindle advances an interpretation of technological change in early America that is bound to command the attention and respect of a wide audience. In doing so, he joins Cyril Stanley Smith and Eugene S. Ferguson (among others) in probing the intellectual component of technological creativity and identifying its origins in the arts of design rather than in science. Historians of technology and those interested in the process by which inventions move from personal to institutional levels will find this book informative and exciting. It is a delight to read.

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MICHAEL R. BELKNAP, editor. *American Political Trials*. (Contributions in American History, number 94.) Westport, Conn.: Greenwood Press. 1981. Pp. x, 316. \$35.00.

If this reviewer were to be as politically just as American governments have often been, he would charge Michael R. Belknap and his nine co-authors with a conspiracy to write a continually interesting, highly readable, historically impressive, and entertainingly dramatic account of the events covered in this book. Is it not seditious to plot such treason against the academic establishment?

While not intending to describe or analyze the entire history of American political justice, Belknap *et al.* have given it a coherence and definition that will serve as an important beginning. Since political trials have proven to be an “inescapable” part of the nation’s tradition, it is high time that this darker side receives the attention it deserves. It symbolizes a partisan turbulence and malicious misuse of justice that has often been lost in the emphasis on constitutional integrity and procedural fairness.

Belknap’s introduction and instructive notes at the beginning of each of the ten essays suggest the range, influence, and cyclical nature of political justice as well as the numerous legal forms its discriminatory application may take. The essays themselves represent this diversity focusing on key, yet sometimes forgotten, cases from Zenger to the Chicago conspiracy trial of 1969. It is a political

theater that has staged everything from treason and sedition indictments to criminal prosecutions for incitement to insurrection with the all-encompassing conspiracy clause always ready at hand.

Although some of the narratives slow down in the details of the courtroom proceedings, the accounts are generally deftly told and very well analyzed. The authors are expert at leading the reader through the maze of shifting political alliances, stratagems, and vendettas that influenced the legal outcomes. One is led to conclude that the weaker the evidence, the stronger the politics and that David never slays Goliath in these unequal encounters. But Goliath does not always win either. Even in an immediate defeat (the two Pennsylvania loyalists tried and hanged for treason), the government's overall restraint on attainment clearly emerges. However vicious the Caster Hanway prosecution in the fugitive slave case, the Fillmore administration failed to expand the law to constructive treason. The Chase impeachment outcome was a victory for moderation, conciliation, and constitutional well-being. Even the wartime fascist sedition cast (*U.S. v. McWilliams*) foundered under the weight of its own political stupidity.

American Political Trials suggests, therefore, that politics and justice do make strange bedfellows since the consequences of their alliance can never be safely predicted. Defendants have also politicized the judicial process and have been saved by it. And the political storms that generate the winds of persecution can also reverse direction as they did in the Milligan case. In a more ominous conclusion, however, Belknap argues that political justice has rarely attacked powerful opponents of the system or those members of mainstream political groups, certainly not since 1800. A politically bad tendency that involves criminal prosecution has increasingly characterized the overt repression of dissent. The tradition of American protest will not remain viable as long as the great bureaucratic machinery of government can come crashing down on those voices of radical independence that challenge the status quo.

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HENRY WARNER BOWDEN. *American Indians and Christian Missions: Studies in Cultural Conflict*. (Chicago History of American Religion.) Chicago: University of Chicago Press. 1981. Pp. xix, 255. \$14.95.

If one wishes to read a concise, thought-provoking ethnohistory of Indian missions, 1540–1980, this is it. Henry Warner Bowden's history, perhaps for the first time, places the sweep of Christian evangelism fully in the context of vigorous, believable, native religions. He also partially succeeds in overcoming a

formidable problem: how to organize hundreds of native cultures meeting dozens of Christian denominations over four centuries. Bowden tries to solve the problem of diversity in a conceptually sound manner, producing a work of insight, good taste, fairness, and excellent cross-cultural comparisons.

The text opens with a summary of pre-Columbian cultures and religions, making the often-neglected point that Europeans did not find a crude form of paganism nor did they discover a virgin land when they moved to North America. The colonial period (occupying two-thirds of the book) obtains focus through three different encounters, the Spanish-Pueblo, the French-Huron, and the English-Massachusett. In each case, Bowden describes precontact native life, discusses the particular form of Christian mission, and points out the results of interaction. He then makes theological and cultural judgments to explain the potential for accommodation and to determine why, in light of that potential, missions failed and tribes disintegrated.

The scheme works well for the colonial period as the author largely restricts his narrative to three tribes. When he begins to cover more ground in the eighteenth, nineteenth, and twentieth centuries, the quality thins a bit. Cherokee removal provides one useful paradigm, but the section on the Dakota falters because Santee missions are used to follow a description of plains culture. Failure to better integrate the Dawes Act, the Indian Reorganization Act, and termination policy with the churches weakens the modern story. The contract school controversy and pervasive cooperation between church and state deserve more attention. Other problems include Bowden's opinions on spiritual causation, Indian alter egos, mental depressions, "downward psychological spirals" (pp. 194, 216), and "collective self-esteem" (p. 220). He treads on firmer ground when explaining mission-native conflict in terms of cultural history. Considerations such as property and material wealth, individualist and communal values, aboriginal religious institutions or their absence, the size and power of political units, and different attitudes toward land, nature, and wildlife at least rest on evidence that can be debated.

Having read this book, one can feel secure in several generalizations, including a caution against easy generalization. Stereotypes about Indians or missionaries appear nearly useless. Bowden also convinces us that tribal religions were as different from each other as they were from Christianity, that Christians frequently confused the gospel with Spanish, French, Dutch, English, or frontier customs, and that the long-term consequence of much mission effort often was social deterioration among Indian tribes. That occurred, he recognizes, despite the good intentions and remarkable courage of such men as Jean de Brébeuf, David Zeisberger, Daniel

Worcester, Isaac McCoy, and Pierre De Smet, admirable men who lived out a passionate commitment to Indian redemption.

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V. V. SOGRIN. *Ideinye techeniia v amerikanskoi revoliutsii XVIII veka* [Ideological Currents in the American Revolution of the Eighteenth Century]. Moscow: Nauka. 1980. Pp. 311. 2 r. 40 k.

Surprisingly, American history has remained until recently a relatively neglected field of study in the USSR. Although an American sector of the Institute of History was established at the Academy of Sciences in 1953, it was not until its reorganization in 1968 as the Institute for the USA that work could begin in earnest. Not until then were political conditions deemed propitious and adequate funds provided for the extensive purchase of American books, subscriptions to American historical journals (as of several years ago the Academy of Sciences could not even boast of a complete run of the *William and Mary Quarterly*), and for the dispatch of Soviet Americanists to the United States in meaningful numbers. And not until 1970 did the Institute begin publishing *SShA: Ekonomika, politika, ideologiia* (The USA: Economics, Politics, Ideology), the USSR's first journal devoted exclusively to American studies. Improved access to scholarly materials coupled with expanded outlets for research findings finally stimulated the creation by the mid-1970s of a body of Soviet Americanists whose works, based upon some familiarity with American sources, are now appearing with increasing frequency. As is to be anticipated, these Americanists have tended to focus their attention on topics that relate most directly to contemporary Soviet concerns: the origins of capitalism, the labor movement, the Civil War, civil rights problems, economic expansion abroad, and foreign policy. What of the Revolutionary era? Some fourteen years ago N. N. Bolkhovitinov, the USSR's leading Americanist, lamented on these pages the absence of "a single serious monograph devoted to the American Revolution" (*AHR*, 74 [1969]:1234). The situation has since changed. Spurred by the professional improvements enumerated above as well as the bicentennial observance, Soviet scholars have recently devoted substantial attention to the War for American Independence, viewed by Marx and Lenin as a true social revolution, one that fits the typological pattern of bourgeois-democratic revolution in the early, manufactory stage of capitalism. The monograph here under review represents a prime example of recent Soviet scholarship on the War for

Independence, and as such reflects its strengths and weaknesses.

V. V. Sogrin's objective is to examine the socioeconomic and political ideas of the Revolutionary era. In keeping with traditional Marxist-Leninist teachings, he declares these to be typical of bourgeois Enlightenment thought that has been emancipated from religious preconceptions ("the ideological antithesis of Protestantism," he labels this thought on page 282, rejecting entirely Henry F. May's schema). Because the colonies had bypassed feudalism, he argues, the American Enlightenment was more consistently bourgeois than its European counterparts. For precisely the same reason, it remained moderate throughout the period covered. Having placed the American Enlightenment within the larger framework, Sogrin proceeds to distribute its leadership element along the political spectrum. On the left wing he locates the republicans, whose ideas varied over time, but who generally believed in popular sovereignty, majority rule, and separation and balance of powers. Within this wing he spies two factions. One confined itself to the demand for political equality, while the other insisted that equality had to be economic as well as political, although the program it espoused left private property essentially intact (for there were to be no Babeufs spawned in the New World). On the right wing of the spectrum were the conservatives, in whom Sogrin evinces little interest because by definition they did not qualify as enlighteners.

The ideology of the American Revolution took root in the mid-1760s with the passage of the Stamp Act, asserts Sogrin, and reached fruition in the debates over the ratification of the Constitution. In its early stages patriotic thought was still moderate. Anticolonialists such as James Otis and John Dickinson came to the fore with their modest demands for limitations on Parliament, although radicals such as Benjamin Franklin and Sam Adams soon opted for home rule. A decade later the radicals had emerged triumphant with demands that in some cases extended to a denial of the king's right to rule. Basing themselves on natural rights and Locke's social contract, they began to justify rebellion against tyranny. By 1776 moderates and radicals alike had been swept into the revolutionary current, as a consequence of which the question of relations with the mother country was replaced by that of domestic economic and political reform. Along the new spectrum that evolved the democrats were those who sought thoroughgoing reform. Spearheaded by the trinity of Thomas Paine, Ben Franklin, and Thomas Jefferson, with contributions also from George Mason, Richard Henry Lee, and Benjamin Rush, the members of the left arrived at the realization that society would have to regulate private property if their program was effectively to be implemented.

Despite their Lockean heritage, they spoke out against slavery and for manhood suffrage, agrarian reform, publicly financed public education, and the expansion of legislative powers at the expense of the executive. Although urban-oriented democrats, they were not prepared to call for price regulation, a failure Sogrin terms "a political mistake" (p. 176). Sogrin dates the emergence of this democratic ideology to the publication of Paine's *Common Sense* in early 1776; the basic components of the ideology were enshrined in the Declaration of Independence that followed half a year later.

Figures such as John Adams, James Wilson, Patrick Henry, Alexander Hamilton, and James Madison, who have been relegated by the Progressives to the ranks of the conservatives, were not true conservatives, Sogrin maintains, because they did not resist all change. Rather, they were moderately bourgeois and participated wholeheartedly in the Revolution, even though they found some aspects of the democratic program too radical. If the democrats with whom they were temporarily allied expressed faith in man's innate goodness, the moderates feared his "daemonic urges." They thought of society as comprised of a property-owning minority and a propertyless majority that would constantly attempt to encroach upon the rights of the propertied. Partisans of liberal economic theory, they thought property inequality not only inevitable but desirable. Only a strong executive branch could provide the necessary security for property, and clearly the Articles of Confederation were inadequate. Pursuing acquisitive objectives by political means, the Northern bourgeoisie joined with the Southern oligarchy to form a "bourgeois-planter bloc." Although the moderates frequently disliked slavery, they surrendered on that score to the wishes of the planters in order to establish a strong central government capable of securing property rights. Frightened by the mass agitation of the postwar period, and in particular by the Shays Rebellion (always a favorite of Soviet Americanists), the bloc broke with its former democratic allies in 1783 to enter into alliance with the remaining Tories. The union was crowned in 1787 with the elaboration of the Constitution. But the victory was not total: for while the Constitution marked a retreat on states' rights, and sanctified private property and slavery, it also curbed arbitrary political power, and thus represented a realization of one of the ideals expressed in the Declaration of Independence. Hence it reflected a compromise not merely between Northern bourgeoisie and Southern planter oligarchy, but between this bloc and the democrats, who still enjoyed widespread support. In portraying the Constitution as a compromise, Sogrin differs not only with Merrill Jensen, with whom he sympathizes on other points, but with many of his own Soviet colleagues.

While the reader may well choose to disagree with Sogrin's findings, he will not complain about the source base utilized. Although the works of Marx, Lenin, Herbert Aptheker, and William Z. Foster are cited, the author has consulted much of the major English-language secondary literature on the subject as well as the standard published primary material. As a Marxist, Sogrin views ideology as a product of domestic class struggle and hence ignores or rejects the approach of Bailyn, Colbourn, Greene, Pocock, Robbins, and others, which traces republican ideology back to the radical Whig tradition in Britain. Sogrin instead looks to the Progressive historians for inspiration. His point of departure is Parrington's *Main Currents in American Thought*, one of the very few non-Marxian secondary accounts of American history to be translated into Russian (1962–63). He does not, however, accept Parrington's (and Beard's) agriculture-commerce bifurcation. The leaders of public opinion were all bourgeois, he argues, and thus prisoners of bourgeois ideology. Sogrin finds congenial Jensen's study of the Articles of Confederation, which traces the democratic thread in American thought back to 1776 and the appearance of *Common Sense* rather than to the 1790s, as Parrington does. (Indicatively, Paine is one of two Revolutionary figures whose works have been published in monographic form in the Soviet Union: the other is Franklin.) E. P. Douglass contributes to Sogrin's synthesis by delineating a democratic political ideology espoused by the Regulators. For the political debates Sogrin draws heavily from Gordon Wood, whose examination of the creation of state constitutions points out ideological divisions based on political principles; he decries, however, Wood's failure to link these principles to socioeconomic thought and eventually to class structure. Finally, Jackson T. Main's investigation of the antifederalists appeals to Sogrin because of its depiction of the rank and file of the movement, although not necessarily its leadership element, as democratic in outlook. From such sources, chiefly Progressive in character, the author has fashioned a synthesis compatible with the Soviet Marxist approach to American history.

Non-Marxist scholars will not necessarily be enamored of the synthesis, which they may well find abstract and schematic. They will not be charmed by his philippics against "bourgeois historiography," especially against the consensus school and its "conscious" attempts to "obscure the class struggle," and will find distasteful his attack on Robert E. Brown, "one of the leaders of the most odious, conservative wing of the consensus school" (p. 40). They will be disappointed that he limits his discussion to socioeconomic and political themes, and ignores such issues as foreign policy and religion. These non-Marxist scholars will also wonder at the author's

compulsive urge to categorize taxonomically all his thinkers and their ideas, much as the botanist classifies a new species by genus and family. But since ideological variation is a direct reflection of class struggle, politico-military rather than arid botanical terminology is called for. Accordingly, the work is replete with (anachronistic) terms such as "camp," "democratic forces," "blocs," "enemies," "positions," "demands," "struggle," and "ideological victory." History in such a vein will prove curious rather than attractive to most non-Marxist historians.

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HARVEY H. JACKSON *et al.* *Georgia's Signers and the Declaration of Independence*. Atlanta: Cherokee. 1981. Pp. 106. \$8.95.

This book consists of political biographical sketches of Georgia's three signers of the Declaration of Independence: Lyman Hall by James Harvey Young, Button Gwinnett by Harvey H. Jackson, and George Walton by Edwin C. Bridges. There are two introductory chapters of background material on Revolutionary Georgia, one by Bridges and the other by Jackson, and there are full genealogies of the three by Kenneth H. Thomas, Jr. The most complete life is that of Walton; there is a real scarcity of personal information available on Hall and Gwinnett.

There is little new information in these sketches, certainly no major new interpretations. Each author has done previous and more lengthy work on his subject. This reviewer finds little to quarrel with in this book. There are a few places where the authors do not show completely the other side of the story and are too favorable to their subjects. Bridges paints a more favorable picture of Walton than this reviewer is willing to accept.

Georgia's signers were born in England (Gwinnett, 1735), Connecticut (Hall, 1724), and Virginia (Walton, 1749 or 1750), but few leaders or soldiers in Revolutionary Georgia were born in the colony (founded 1733). Gwinnett was the son of a clergyman, and Hall came from a clerical family. Walton was the son of a poor man and raised as an orphan. Hall attended college (Yale) and was briefly a Congregational minister. Gwinnett and Hall were both from St. John's Parish, a center of former New England Congregationalists, down the coast from Savannah. Gwinnett was an unsuccessful merchant and planter, Hall a successful planter and doctor. Walton was a very successful young Savannah lawyer and reputedly the youngest man (twenty-six) to sign the Declaration.

All three were successful politicians. Gwinnett and

Walton entered fully into the partisan politics in the Georgia of their day. They were both in the center of controversy while alive and since—Walton was probably the most controversial as he lived the longest (until 1804) and was continually engaged in politics. Hall was probably the least controversial and most "statesman like" of the three. Certainly he saw Georgia's postwar needs better than most politicians.

It is well to have this volume, which gives the heart of what is known about Hall, Gwinnett, and Walton in one convenient place—something that was not true for any of the men previously. Kenneth Thomas's genealogies should settle any doubts about ancestors or descendants.

KENNETH COLEMAN

University of Georgia

STEPHEN A. MARINI. *Radical Sects of Revolutionary New England*. Cambridge: Harvard University Press. 1982. Pp. 213. \$16.50.

This carefully crafted book, a study of the first major manifestations of indigenous American Protestantism, adds significantly to the standard accounts of sectarian religion in Revolutionary New England by C. C. Goen and William G. McLoughlin. It tells how the radical followers of George Whitefield who migrated into frontier New England during the 1770s established Christian sects that by 1815 were together as numerous in the region as Congregationalists and Baptists. Stephen A. Marini charts the course of ephemeral bodies like the Come-Outers, Merry Dancers, and Nothingarians but concentrates on common patterns of development in the most successful new sects—Free Will Baptists, Universalists, and Shakers. These groups emerged when charismatic leaders successfully exploited the "New Light Stir" that spread revival across rural New England from 1778 to 1782. They experienced crises of leadership and organization in the mid-1780s. They achieved institutional stability in the 1790s. They added new converts, defined their theologies, and developed forceful hymnodies early in the new century. By 1820 they had begun to resemble other denominations with traditions and bureaucracies of their own. Although the sects provided social order for a region that was strained by economic, political, and military disruption, the three major groups owed their success primarily to the power of their spiritual vision. The result was a new religious landscape in which millennialism, perfectionism, and anti-Calvinism had become institutionalized in the associational "gospel union" of the Free Will Baptists, the minimalist "gospel liberty" of the Universalists, and the celibate "gospel order" of the Shakers. Marini's grasp of the internal

history of these groups is masterful, and his sketches of a host of dramatic leaders—like the Canadian itinerant Henry Alline, the Free Will Baptist Benjamin Randel, Universalists John Murray, Elhanan Winchester, Caleb Rich, and Hosea Ballou, or Shakers Ann Lee, Joseph Meacham, and Lucy Wright—are superb.

Some questions remain, particularly concerning Marini's stress on the radicals' anti-Calvinistic character. In point of fact, the sectarians' insistence on free will and their view of the atonement as governmental and universal were not greatly different from convictions that by 1800 were coming to prevail among the acknowledged heirs of Jonathan Edwards. While separated by contrasting dispositions, sectarians and established leaders participated together in the adjustment of biblical, revivalistic Evangelicalism to the new set of cultural presuppositions provided by the ideology of Revolution and the intellectual assumptions of a domesticated Enlightenment. Thus, while Marini perceives correctly that the New England sects "embodied the cultural ferment of the Revolutionary era" (p. 135), he does not see as clearly that this particular embodiment was merely part of an accommodation taking place in every facet of American life. Marini rightly describes the sectarianism of Revolutionary New England as an anticipation of the Disciples, Mormons, and Adventists that were to come. But it may also be true that sectarianism was not, as he suggests, a counterpoint to religious development in the traditional churches so much as the advance guard of a march on what they too had embarked.

MARK A. NOLL
Wheaton College
Wheaton, Illinois

ERNEST MCNEILL ELLER, editor. *Chesapeake Bay in the American Revolution*. Centreville, Md.: Tidewater. 1981. Pp. xxxv, 600. \$29.50.

During the bicentennial, copies of archival materials dealing with Chesapeake Bay during the Revolution were gathered at Washington College. The authors of this collection draw heavily upon this repository to produce fourteen essays on maritime-related topics. Ernest McNeill Eller, the editor, contributes introductory and concluding chapters that survey events leading up to the Revolution and military activity in the bay area, including an especially lively account of the Yorktown campaign. Between Eller's two essays four chapters chronicle specific British operations on the bay, three examine the conduct of state navies and American privateers, and four describe the impact of the war on the region's shipbuilders and merchants. Together the essays provide an engaging portrait of the many and

varied maritime activities in an area that was only briefly the center of military operations but one that was important, the editor believes crucial, to the conduct of the war even before the climactic Franco-American victories in the battles of the Virginia Capes and Yorktown.

Solidly based on primary sources, these well-written essays clearly demonstrate the impact of the Revolutionary War on areas away from the center of political and military activity. Todd Cooper's chapter on the effects of wartime government, inflation, and enemy naval action on Baltimore's trade and the response of the city's merchants is excellent. The history of the Maryland state navy by Myron J. Smith and John G. Earle is the best account of that organization to date. The chapter on the Virginia state navy contains as much descriptive detail but lacks the sophisticated analysis. Edwin M. Jameson's study of Tory maritime operations reflects the large degree to which the Revolution was a civil war in the South and reminds the reader that military action did not cease with the surrender at Yorktown.

All of the authors maintain a high degree of objectivity. Lord Dunmore, who is often depicted as an arch-villain by American writers, emerges as a reasonable man doing his best in a difficult situation. Indeed, by saying little about the many raids conducted against plantations by Dunmore's forces, Alf J. Mapp gives him, perhaps, more than is his due. Only Nathan Miller's "Chesapeake Bay Ships and Seamen in the Continental Navy" verges on the filiofetism that mars many local studies. Few of the operations described in the chapter originated in the bay or had any direct impact on the area, and some of the individuals whose exploits are chronicled (for example, John Paul Jones whose activities consume almost a fifth of the chapter) had only the most tenuous link to the Chesapeake.

Though some of the chapters address topics of national importance, the collective result may fail to convince readers of "the dominant role of this inland sea and bordering lands in achieving independence" (p. xiii). The authors do clearly demonstrate the value of viewing the Chesapeake as a unit and thereby suggest the need for other regional studies. By examining the minutiae involved in the daily operation of government, they vividly convey a sense of the mundane and frustrating problems involved in providing defense and operating forces in an area away from the main theater of war. This volume contains good reading for the professional historian and general public alike.

JAMES C. BRADFORD
Texas A&M University

JACOB ERNEST COOKE. *Alexander Hamilton*. New York: Charles Scribner's Sons. Pp. vi, 277. \$17.95.

No one familiar with Jacob Ernest Cooke's track record will be surprised to learn that this biography of Alexander Hamilton is carefully researched, meticulously crafted, and (despite its brevity) thorough. The author's years of work on the Hamilton papers, his exhaustive biography of Tench Coxe, and his brilliant articles on various controversial episodes of the 1790s are all in evidence in the current study.

Cooke is ruthlessly and almost painfully impartial in his judgments of Hamilton and his contemporaries. As a result, though Washington emerges virtually without a blemish, nearly everyone else is left with his imperfections, ranging from mild pockmarks to leprotic lesions, pitilessly exposed. Hamilton's personal flaws are dwelt upon at length; perhaps indeed the length is excessive, though Hamilton's impeccable public conduct, at least until 1799, is, through the contrast, made more awesome. Hamilton's enemies do not fare so well from equally balanced treatment: Jefferson and Adams, especially, turn out to be nearly as bad as Hamilton thought they were. Along the way, Cooke demolishes myth after myth propagated by historians partial to Jefferson, Madison, and Adams—myths concerning the compromise of 1790, Hamilton's alleged phalanx of kept congressmen, his "improper" interference in foreign affairs, his roles in the Whiskey Rebellion and the negotiation of Jay's Treaty, his supposed control of Adams's cabinet, and his "conspiracies" in 1798, to name but a few. The textbook writers, if they are to keep their work honest and up to date, will be impelled to make a thorough overhaul of their chapters on the Federalist era in light of this book.

There are, however, some weaknesses. Interpretively, Cooke adheres fairly closely to John C. Miller's 1959 "Portrait in Paradox" treatment (which he curiously refers to as a "recent" biography). Far from being paradoxical, inconsistent, or erratic in his behavior, Hamilton was astonishingly consistent from start to finish, if allowance is made for the fact that he grew progressively wiser and more mature through experience and study. To comprehend this, one needs only abandon an "objective" approach and view his words and deeds from the perspective of his own values and attitudes—apropos of which, it must be observed that Cooke's biography is almost totally devoid of discussion of what Hamilton read and was influenced by. Instead, there is a great deal of neo-Freudian nonsense that attempts to explain the imagined paradoxes. For instance, in considering Hamilton's unfortunate attraction to Maria Reynolds, Cooke speculates that this wanton lady somehow reminded Hamilton of his long-dead mother. (Who can say what chemistry impels a man to make a fool of himself over a particular woman even when he knows better? Henry VIII turned a kingdom upside down out of

sexual beguilement by Anne Boleyn. Was there buried deep in Henry's psyche a craving to sleep with his mother?) Finally, Cooke simply does not understand Hamilton's financial program, the revolutionary essence of which was the monetization of the public debt. Since Hamilton's greatest achievements turned upon his financial policies, this shortcoming would be fatal in a work that was not otherwise so solid.

FORREST MCDONALD
University of Alabama

MARIS A. VINOVSIS. *Fertility in Massachusetts from the Revolution to the Civil War*. (Studies in Social Discontinuity.) New York: Academic Press. 1981. Pp. xii, 253. \$27.50.

Methodologically and conceptually sophisticated, this tour de force of early nineteenth-century American demography is a disarmingly simple book to read. Almost all of the apparatus is placed in appendixes and even those who are intimidated by demographic history will have no trouble following the step-by-step analysis. The first chapter identifies the dimensions of a trend well-known to demographers, that of declining fertility rates in the early republic; the subsequent eight chapters attempt to explain why this decline occurred. Maris A. Vinovskis is ruthless in pursuing and measuring socioeconomic variables, which he regresses against the dependent variable of declining fertility, and he is painstakingly careful in evaluating the reliability of his data. The result is one of the most important, tightly argued, and readable books to be published on American population history.

The relationship between fertility and social structure is complex. Vinovskis argues that most of the previous case studies that have explored the relationship and relied on aggregate data for states or individual communities have overemphasized specific variables. For example, historians frequently argue that a declining availability of land and an increase in urbanization and industrialization account primarily for the decline in fertility. Although the decline in fertility does correlate with these variables, Vinovskis contends that the correlation is much weaker than is generally thought: rural, non-industrial towns in Massachusetts with substantial amounts of available land also experienced a decline in fertility rates. Similarly, he argues that imbalances in the ratio of men to women and a growing ethnic heterogeneity affected fertility rates less than might be expected. The strongest predictor of fertility rates was education; the more education one had, the less likely one was to have many children.

The inability of his data on socioeconomic conditions to account for most of the decline in fertility,

and the high correlation between the decline and education, leads Vinovskis to argue that historians have overemphasized economic causes in explaining American demographic trends and underemphasized cultural forces. Paradoxically, he assembles a mass of hard data to prove that hard data alone cannot explain shifts in population history. Moving from multiple regression to qualitative analysis, Vinovskis speculates that a new personality type that emerged in late eighteenth-century American society contributed much to the decline in fertility. An increase in romantic religion and a corresponding decline in fatalism, coupled with an increasingly high level of individual aspiration, led people to make conscious decisions to limit their fertility. Although he does not state so directly, Vinovskis is implicitly arguing that Revolutionary, Jeffersonian, and Jacksonian ideology had as important an effect on population patterns as did the economic facts of life.

One cannot begin to do justice to such an important book as this in a short review. It has at least five virtues: (1) it provides answers to many difficult questions; (2) it is an extraordinarily helpful methodological model; (3) it explicitly identifies research opportunities; (4) it is one of the few major works in demography that will be useful in undergraduate teaching; and (5) its conclusions suggest to a world, teetering on the brink of widespread famine due to overpopulation, some of the conditions that are conducive to population control.

BRUCE C. DANIELS
University of Winnipeg

J. GERALD KENNEDY. *The Astonished Traveler: William Darby, Frontier Geographer and Man of Letters*. Baton Rouge: Louisiana State University Press. 1981. Pp. xiii, 238. \$22.50.

Reared in the backwoods of Pennsylvania during a period of ceaseless Indian wars, William Darby (1775–1854) became an inveterate traveler, well-known geographer, and self-taught man of letters. At the age of twenty-two he migrated to Louisiana where he later became a United States deputy surveyor. His knowledge of the Old Southwest region was put to good use by Andrew Jackson during and after the Battle of New Orleans. Later, the United States and Spain relied heavily upon his previous reconnaissance work in fixing the boundary between Texas and Louisiana Territory. Darby returned to the East in 1815 to find a publisher in Philadelphia. By now he was a middle-age widower, unemployed, and the father of a young daughter whom he had relinquished to the care of friends and would never see again. He eventually found a publishing outlet for his *Geographical Description of*

the State of Louisiana and subsequent *Emigrant's Guide to Western and Southwestern States and Territories*.

According to J. Gerald Kennedy, Darby's *Geographical Description of the State of Louisiana* "made a genuine addition to geographical knowledge, for until his study, the terrain and boundaries of that state were but imperfectly known. An examination of the original map reveals the cartographer's care for accuracy: each river, lake, and bayou is meticulously delineated. The achievement is the most remarkable when we realize that Darby was probably the first man of science to explore some areas of Louisiana" (p. 57).

Through most of his "chequered existence," Darby had little to show for his labors as a geographer, lecturer, traveler, and author of books and stories, and as a gazetteer. He seemed always to have been trapped in an inescapable pattern of catchpenny drudgery and debt. He "never wrote a truly memorable book, nor did he exert much influence on younger geographers and historians" (p. 126). His literary and scientific contributions eventually faded from human memory. Yet, for all of his plodding and meticulous compilation, he was a man of vision, driven by a passion for knowledge and a determination to promote the settlement of the American West.

The first 128 pages of this book are devoted to the life of William Darby, based upon what sketchy materials Kennedy uncovered during a five-year search. Part 2, "The Magazine Fiction," contains two of Darby's tales that would have been better left unresurrected, except that they reflect upon some of the literary conventions prevalent during the early nineteenth century. Part 3, "Geographic Writings," features excerpts from *A Geographical Description of the State of Louisiana* and *The Emigrant's Guide*, among others. Although he deserves considerable credit for his painstaking detective work in unmasking the geographer Darby as the author of numerous frontier adventure tales written under the pseudonym of Mark Bancroft, one is inclined to ask "so what?"

But to his credit, Kennedy, an associate professor of English at Louisiana State University, does a good job with the materials at hand. He writes in a clear, concise style and demonstrates a knowledge and understanding of the Western movement.

W. EUGENE HOLLON
Santa Fe, New Mexico

JAMES ALEXANDER GARDNER. *Lead King: Moses Austin*. St. Louis: Sunrise. 1980. Pp. 249. \$9.95.

This book attempts to remove Moses Austin (1761–1821) from the historical shadow cast by his more famous son, Stephen. According to James Alexan-

der Gardner, Moses made regional and national contributions through his pioneering work in lead mining, first in Virginia then in Missouri, and by his plans to colonize Spanish Texas. In seventeen short chapters (including two of five pages), the subject emerges as an ambitious businessman-entrepreneur who looked to the frontier as an escape whenever his economic fortunes began to fade.

Born to a tailor in Durham, Connecticut, Austin began mining in Virginia after the Revolutionary War when he acquired mineral lands in Wythe County. He improved the methods of mining and smelting galena ore and constructed a factory to make lead shot. Labor and money shortages combined with conflicts with the state government over unfulfilled contracts, however, damaged the enterprise. Gardner ascribes this and subsequent failures to a "tragic flaw" in Austin's character: "trusting unworthy associates and striving to engage in too many projects simultaneously" (p. 34).

Having moved his family to St. Genevieve in Spanish Upper Louisiana in 1798, Austin achieved wealth and regional prominence by developing lead deposits at Mine à Breton. The book details his success in mining and manufacturing lead and his participation in local projects, including land speculation, an academy, the town of Herculanum where he erected smelters and a shot tower, and the first bank in territorial Missouri. Gardner relates Austin's difficulties in gaining clear title to his lands from Spanish authorities, and later his political quarrels with American Governor James Wilkinson, filibustering ally of Aaron Burr. The War of 1812 depressed the lead business and impaired his other ventures, turning the old man's thoughts to Texas, where he hoped once again to realize his irrepressible yearnings for success.

A revised 1963 dissertation, Gardner's study bears the marks of its genre: adequate research but weaknesses of organization and style. The chronology is not always clear, and material is sometimes repeated; the list of documents supporting Austin's land claims appears on page 82 as well as page 89. Too often the prose is numbing, "... his personal library reflected another side of his intellect, namely his catholicity in so far as his reading was concerned" (p. 135). Nor is the analysis penetrating. Although we see glimpses of Austin as either a shrewd, manipulating individual or a victim of circumstances beyond his control, his character remains ill defined. Little is revealed about the "potent influence" (preface) Moses supposedly had on Stephen. The publisher left an unfortunate number of typographical errors, failed to indent some paragraphs, and provided an index listing only proper names.

This book will be useful to historians of Missouri and specialists in early mining history. But Gardner

misses the chance to elevate Austin to national significance for his role in westward expansion.

JOSEPH E. KING
Texas Tech University

CHESTER L. KIEFFER. *Maligned General: The Biography of Thomas Sidney Jesup*. San Rafael, Calif.: Presidio Press. 1979. Pp. xiii, 376. \$16.95.

Chester L. Kieffer has written the first biography of Thomas S. Jesup, an important figure in the early history of the U.S. Army. Raised on the Kentucky frontier, Jesup entered the army in 1808 and earned a reputation for gallantry during the War of 1812, especially as an infantry officer in the hard-fought Niagara campaign of 1814. While on recruiting duty in Connecticut late in the conflict, he kept the Madison administration informed of the progress of the Hartford Convention. Appointed quartermaster general in 1818, he was one of a group of able young staff officers who, under the supervision of Secretary of War John C. Calhoun, reformed the army's supply services, ending the confused and decentralized logistical practices that had obstructed wartime operations. Jesup administered the Quartermaster Department with competence and integrity until his death in 1860. He proved less successful as a field commander in the army's efforts to remove the Creek Indians in 1836 and the Seminoles between 1836 and 1838. His conduct of the latter campaign, especially his violation of a flag of truce to seize the Seminole leader, Osceola, and other Indians, aroused widespread public criticism at the time and has plagued his reputation to the present.

Jesup's career illustrates the transitional state of the military profession in the antebellum era. As quartermaster general, he was a modernizer, a leader in the broader trend in the nineteenth-century military establishment—and in public administration generally—toward uniformity, efficiency, and rational bureaucratic procedures. His success is reflected in the comparative performances of the supply services in the War of 1812 and the Mexican War. At the same time, Jesup displayed the touchy individualism and disputatiousness that had pervaded the officer corps of the Revolution and the early national period. While stationed in Louisiana in 1816, he longed for a war with Spain and on his own initiative planned an attack on Cuba. Throughout his career, he maintained close ties with political leaders; his quarrel with Winfield Scott over the conduct of the Creek war became entangled in party politics and ran for years in Congress and the press.

Kieffer, a retired U.S. Army historian, bases his study on Jesup's extensive but largely neglected

personal papers in the Library of Congress and the official records of the Quartermaster Department. His principal aim, reflected in his title, is to rescue Jesup's historical reputation from his critics. While he corrects misconceptions, he relentlessly defends the quartermaster general in his many controversies and explains away evidence that detracts from his portrait of Jesup as a devoted professional, victimized by self-serving and partisan foes. Perhaps the least convincing aspect of Kieffer's study is his treatment of the Seminole campaign, especially his justification of the capture of Osceola by reference to the previous bad faith of the Indians. John K. Mahon's standard *History of the Second Seminole War* (1967), a work not listed in Kieffer's bibliography, includes a far less favorable account of Jesup's Florida command. Despite its advocacy tone, *Maligned General* is a well-researched and clearly written study of a neglected figure and deserves the attention of students of nineteenth-century American military history.

WILLIAM B. SKELTON
University of Wisconsin,
Stevens Point

JOHN DENIS HAEGER. *The Investment Frontier: New York Businessmen and the Economic Development of the Old Northwest*. Albany: State University of New York Press. 1981. Pp. xviii, 311. Cloth \$39.00, paper \$12.95.

CARL ABBOTT. *Boosters and Businessmen: Popular Economic Thought and Urban Growth in the Antebellum Middle West*. (Contributions in American Studies, number 53.) Westport, Conn.: Greenwood Press. 1981. Pp. xii, 266. \$27.50.

A Jeffersonian-Populist bias against businessmen and speculators is not hard to find among historians of the American frontier. For such views, these two new books on the Old Northwest before the Civil War are both antidote and corrective. John Denis Haeger concentrates on the contributions of a few New York businessmen to the economies of that developing frontier from Ohio to Michigan. Carl Abbott analyzes the effects of popular business thought in four cities in the same region. In both cases the business community is interpreted and described sympathetically.

John Denis Haeger, economic historian and product of the Middle West, deals primarily with Arthur Bronson and Charles Butler, two wealthy entrepreneurs. The Bronson family sat with Stuyvesants, Van Rensselaers, and Astors, and helped form institutions like the New York Life Insurance and Trust Company. Their fellow shareholders included John Vanderbilt and Washington Irving.

When Bronson and Butler turned their attention to the Western territories in the 1830s, they carried with them experience in banking, trusts, and land acquisition. Eventually, they traveled and lived in various areas of the Old Northwest, carefully identifying profitable centers of investment like Chicago and Detroit. Realizing the importance of the legal and political setting, they lobbied legislatures for a more congenial business environment. Their contacts extended as far as Martin Van Buren in Washington. Railroads, canals, and other internal improvements figured in their empire. Through these men Eastern and English capital funneled into the locality. One fascinating aspect of the story is the different ways in which the two men approached their business world—Bronson with conservative, cautious balance, Butler with stereotypically Western speculative flair. In the depression of 1838 Bronson's investments escaped practically unscathed, but Butler's fortune suffered seriously.

Carl Abbott, urban historian, in *Boosters and Businessmen* also finds a variety of responses, but his spectrum appears in the group attitudes of four cities—Cincinnati, Indianapolis, Chicago, and Galena. Local attitudes influenced railroads, trade patterns, and markets. He finds, for example, that most boosters were surprisingly well informed on the economic statistics of their cities, including the agricultural hinterlands. In the competitive race, however, Chicago and Indianapolis earned high marks for level-headed cooperation and forged ahead. Cincinnati and Galena, riven with conflicts, were economically declining by the end of the 1850s. Cincinnati had become elitist and too prosperous; Galena could not resolve the drain of its miners after 1849.

These authors have written from an impressive array of sources and primary investigations. Abbott includes an appendix on his sampling procedure. Haeger has consulted a mass of manuscripts in collections from the Chicago Historical Society to the Harvard library. The remarkable aspect is the way these sound and scholarly studies complement one another. They illuminate two sides of an economic coin. Haeger stresses the individual while Abbott emphasizes the group. Each avoids oversimplification by showing a wide variety of responses in a business community. Haeger exposes differences in economic style between men like Bronson and Butler; Abbott relates a number of urban choices determining the rise and demise of whole cities and regions.

ROBERT V. HINE
University of California,
Riverside

AGNES BRETTEING. *Soziale Probleme deutscher Einwanderer in New York City, 1800–1860*. (Von Deutsch-

land nach Amerika, number 2.) Wiesbaden: Franz Steiner. 1981. Pp. x, 224. DM 46.

Agnes Bretting's well-researched portrait of "social problems of German-[speaking] immigrants to New York City, 1800–1860" intends to contribute to the "complex phenomenon of immigration from Europe and acculturation in America" (p. vi). Guided by Robert Ernst's work, *Immigrant Life in New York City, 1825–1863* (1949), the author explores four main subjects. First, she sketches New York as port of arrival, discusses the effects of the sizable immigration of the 1840s and 1850s on the city, and features entrepreneurial and legal dimensions relating to antebellum immigration to New York. Next, she deals with problems immigrants had to master on arrival, especially the deceit of runners, innkeepers, and ticket agents, and outlines private and public assistance offered to needy newcomers. The author then presents the social divisions of New York's German-speaking community: the entrepreneurial and professional group, people of middle incomes, and the impoverished, often victims of a costly journey and of heartless exploitation. The final section focuses on "acculturation." Bretting reviews the role of the churches, especially the Lutheran, Reformed, and Catholic, then discusses the impact of German-language schools and of newspapers of varied ideological stance. She concludes with a portrait of voluntary associations, devoted to benevolence or sociability, and with a discussion of the political affiliations of German-Americans and their enmeshment in the labor movement. The conclusion summarizes the author's interpretive findings. She asserts that Germans had left for economic reasons and "expected nothing more from Germany" (p. 172). Nor had they found reasons to resist Americanization; they made valiant efforts to learn the English language, the main barrier to that end, but became recalcitrant because of nativist attacks. As to their economic success, Bretting maintains that a few became wealthy, many were exploited and destroyed, most, however, were "enabled to create a secure livelihood" (p. 173).

The strengths of the study are clear exposition, the discovery of new data, and the presentation of a cohesive overview. Of special value are the twenty-one appendixes that offer primary information on such topics as newspapers, voluntary associations, and the numbers of German-speaking inmates in poorhouses.

The limitations of the study are threefold. First, at times Bretting selects her material at random and fails to assess it critically and to put it into the proper context. Second, she does not consider secondary works relevant to her themes, such as Frank Thistlethwaite's critical essay, "Migration Overseas in the Nineteenth and Twentieth Centuries,"

(Comité des Sciences Historiques, *Rapports*, 5 [1960]: 32–60), Philip Taylor's *The Distant Magnet* (1971), Peter Marschalck's *Deutsche Überseewanderung im 19. Jahrhundert* (1973), and Milton Gordon's *Assimilation in American Life* (1964). Third, the author overlooks the fact that immigrants were far better informed than she assumes, relied far more effectively on support networks based on kinship or village ties, and were far more complex in their attitudes toward their homeland and the world of New York City. In sum, Bretting's work is limited in analytical sophistication but valuable and welcome as a lucid presentation of primary data.

LEO SCHELBERT
University of Illinois,
Chicago Circle

EDWARD K. SPANN. *The New Metropolis: New York City, 1840–1857*. (Columbia History of Urban Life.) New York: Columbia University Press. 1981. Pp. xiii, 546. \$19.95.

This is a grand, big, booming book. Edward K. Spann has examined the generation before the Civil War when New York City reached its destiny of becoming one of the world's greatest cities, seaports, and commercial emporiums. There is not much that he misses. He operates like a historical vacuum cleaner, picking up the big as well as the smaller pieces: the failures and complexities of urban politics (his portrait of Fernando Wood, for example, is balanced and intelligent), the autocracy of commerce and its lure, the triumphs in culture and technology are fleshed out with sections ranging from markets, hotels, restaurants, housing, the mystique of Broadway, to crime, water, women, transportation, health, and a lively discussion of the Bowery B'hoys, to mention a few. The research is prodigious, the sources are diverse and imaginatively used, and the command of the material is superb.

Two themes predominate, one could have been used as a subtitle: a tale of two cities, the city of progress and the city of poverty, both of which made metropolitan New York the cutting edge of a new urban network in America. The other is the "intense individualism" of New Yorkers, reflected in their myopic self-interest—if not greed—their lack of discipline, their cavalier acceptance of filth and misery, and their insensitivity to the dingy lives of the poor. Only occasionally were New Yorkers touched by a sense of collective, civic responsibility.

Here, then, is an old-fashioned urban biography, especially so because it is written with consummate literary flair, a quality conspicuously absent in the literature of American urban history in recent years. Undoubtedly this influenced the New York Historical Society when it gave Spann its Manuscript

Award. Here, too, is a book that will delight scholars and "civilians" alike. Indeed, *The New Metropolis* deserves a place on the shelf of distinguished urban biographies.

ALEXANDER B. CALLOW, JR.
University of California,
Santa Barbara

JAMES P. HANLAN. *The Working Population of Manchester, New Hampshire, 1840–1886*. (Studies in American History and Culture, number 29.) Ann Arbor: UMI Research Press. 1981. Pp. xxi, 237. \$31.95.

In the spirit of the "new social history," James P. Hanlan examines the working class of nineteenth-century Manchester, a textile town planned and dominated by the Amoskeag Manufacturing Company. Hoping to supplement Tamara Hareven's work on Manchester in the twentieth century, he focuses on the city's community life and on the experiences of its working population, especially the unskilled, over more than four decades.

Hanlan uses an impressive assemblage of local materials, both traditional and quantitative, to describe people as they actually thought and acted. Unlike the typical town history or biographical directory, which emphasizes the achievements of prosperous community figures and ignores the poor, this study reveals the latter as significant shapers of their environment. Here is a culturally diverse people whose differences, for example, retarded the development of a feeling of class consciousness. They were frequently injured on the job, received low wages, and held little property, but their public protests were minimal. Divided along several lines, the unskilled kept adjusting to changing conditions in order to survive.

Amoskeag's attitude toward its work force and the town was paternalistic but also flexible when necessary. Housing is a case in point. Although the rule was that workers were required to reside in company housing, many did not. Instead they lived in their own neighborhoods, where they "could maintain their own unique traditions, customs, and cultures." Wisely, the company tolerated this practice, thereby permitting the workers to determine at least their own residences and precluding potential protest.

Of the factors that divided the textile "operatives" of Manchester, immigration and ethnicity were the most important. With considerable skill, Hanlan traces these factors from the 1840s through the 1880s. He notes that the coming of Catholics in the 1840s caused a nativist reaction among native-born householders. The latter, who were still dominant in Manchester by the 1860 census, would afterward be in the minority. In 1868 and 1870 a total of eighty-five "Scots girls" weavers arrived. Recruited by the

company, they constituted an elite worthy of Amoskeag favoritism. In contrast, the far more numerous unskilled workers, holding the lowest paid jobs, struggled for survival and eventually felt some sense of labor militancy. A factory situation that management had earlier called "a kind of industrial Garden of Eden" in reality became very unpleasant.

Immigrant workers suffered even outside the work place. Natives regarded public services for the newcomers as too expensive. Thus, whenever budgets for education and police protection were considered, they received exceptionally close scrutiny. On the other hand, the fire department, which was important to Amoskeag, was treated very well.

Hanlan's study is a model of thoroughness. His sources include company and town records, federal manuscript censuses, diaries, and personal papers. The information drawn from these sources is displayed in 5 appendixes, 112 tables, and a tightly written narrative. This book is an important contribution to scholarship.

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ANNE C. ROSE. *Transcendentalism as a Social Movement, 1830–1850*. New Haven: Yale University Press. 1981. Pp. xii, 269. \$22.50.

Many good things can be said about this book. Anne C. Rose has done comprehensive research in the sources. The bibliography is vast. The footnotes, cited conveniently at the bottom of the page, are complete and explicit, sometimes consisting of short essays expanding upon the text. The organization of six chapters takes the reader from the origins of Boston Unitarianism in the 1790s through an epilogue about mid-century Transcendentalism. It is a convenient source of information about the activities of leading Unitarians of the first generation, interesting correspondence between familiar figures in the Transcendental movement, and details of the origin, organization, and demise of the communal experiments of Brook Farm and Fruitlands. Rose writes with clarity and confidence, harboring few doubts as to her conclusions. As an outgrowth of a Yale dissertation in American studies, the author is to be commended for her monumental effort.

There is a difficulty, however, that this reviewer could not overcome in reading this book. It does not really hold together. Lack of continuity in thesis development, ambiguous and, at times, over-generalized conclusions result in a work that does not contribute to a better understanding of Transcendentalism. The author sets up a straw man and proceeds to knock it down. She claims the book is

"frankly revisionist" (p. viii). For too long, she implies, scholars have presented the Transcendentalists as aloof, disengaged intellectuals. Rose then sets out to prove them to be active social reformers. Serious scholars of Transcendentalism have long known that some were indeed disengaged intellectuals but that many others were active in social reform. Some were both.

The author also tries to present "The Transcendentalists," a term used throughout, as a cohesive reform group. They never were. Therefore, it cannot be said that "they" failed to achieve their goals by the 1850s when, as the author claims, social reform began to recede with the deepening of the slavery crisis. Similarly, it is difficult to follow the author's thesis that Unitarianism, with its Transcendental outcropping, was a response to "pre-industrial" (p. 9) conditions in Boston, feeling the impact of foreign migration and the growth of sectarianism. If so, why did not Transcendentalism emerge in New York City and Philadelphia? Perhaps Rose, in her attempt to bring in socioeconomic forces (valid enough if relevant), de-emphasized the intellectual roots of Transcendentalism.

No, "The Transcendentalists" never formed a social movement. They were intellectuals and models of ethics. Some, as individuals, were active social reformers. If they failed at social reform, as Rose claims, Transcendentalism ended in 1850. But if Transcendentalism is viewed as an intellectual movement, it had a second generation in the Free Religious Association that lived until 1938, and at the Concord School of Philosophy in the second decade after the Civil War. Here is its link with such people as William T. Harris, William James, and John Dewey. As an intellectual movement it had more permanent influence on the American mind and ethic including women's rights than in any social movement. Yet, this book has value in attempting to look at Transcendentalism in a new framework. That the thesis is a spurious one is probably not so fatal when, to her credit, the author at last recognizes that "the record of Transcendentalism as a movement of great humanity must remain of lasting value" (p. 275).

J. WADE CARUTHERS
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CURTIS T. HENSON, JR. *Commissioners and Commodores: The East India Squadron and American Diplomacy in China*. University: University of Alabama Press. 1982. Pp. vi, 231. \$19.75.

Curtis T. Henson, Jr.'s study of the East India Squadron and American diplomacy in China (1835–61) parallels and complements nicely Gerald Graham's recent book on the Royal Navy and

British diplomacy, *The China Station: War and Diplomacy, 1830–1860* (1978). Henson's thesis is rather straightforward. The East India Squadron, established in 1835, was designed to fulfill the traditional naval role of protecting American life and property abroad and promoting the national interest by showing the flag and extending the authority of the United States government. That task was complicated by China's antiforeign attitude and the declining power of the Ch'ing dynasty, which made a futile effort to resist the opening of the Middle Kingdom by the West in the mid-nineteenth century. Of its three constituencies—missionaries, merchants, and diplomats—only the navy's service to the string of commissioners dispatched to China was really important. Indeed it was vital. The navy brought the diplomats to the Far East, moved them from place to place in fitting style and dignity, and bolstered their demands for treaties, adherence to their terms, and treaty revision. In the minds of the commissioners and commodores, if not always in fact, a display of power was necessary to negotiate, and they frequently asked Washington for an enlarged squadron. Above all, the squadron gave the diplomats mobility and, in view of China's instability and refusal to grant diplomatic residence in Peking until 1860, served as a floating legation.

Henson's contribution is to trace the relationships between the various commissioners and commodores and to evaluate their impact on the conduct of United States diplomacy, something that Robert Johnson generally fails to do in his narrative, *Far China Station: The U.S. Navy in Asian Waters, 1800–1898* (1979). Those relations were often troubled, as the discussion of the Vorhees-Balestier affair and Humphrey Marshall's conflicts with Commodores John Aulick and Matthew C. Perry amply demonstrate. Personalities were sometimes responsible. More important were the demands made by the diplomats on the limited resources of the squadron, which had other duties to perform. The lack of diplomatic-naval coordination was, in turn, the fault of policy makers in Washington, who were wrapped up in domestic matters and who were only marginally concerned with events in distant China. Inaction in Washington was also the result of the traditional attachment to commercial diplomacy and opposition to military intervention in China. On the whole, however, both the diplomats and commodores agreed with the basic thrust of American policy, though they shared a common frustration with Chinese deviousness and American neutrality during the Arrow War. To put things in perspective, the British, as Graham illustrates, were plagued by similar problems.

For the most part, Henson achieves his purposes. Nor does he hesitate to offer assessments of the various diplomatic episodes and the leading diplo-

matic and naval figures, though some of his portraits are flat. His interpretations are largely conventional, but he does credit Commissioner Alexander H. Everett with suggesting cooperation with other Western powers as early as 1847. Henson's treatment of the subject is slimmer than Graham's, but then America's involvement was much more restricted than Britain's. On the other hand, he is less biased against the Chinese and is critical of the ancient delusion that China regarded the United States as a special friend. Henson occasionally exaggerates the "nonpolicy" of the United States in China; a decision to remain passive and neutral also constitutes a policy.

Commissioners and Commodores is based on a thorough investigation of American primary sources but relies on secondary works for the British and Chinese sides of the story. Henson must know that the independence of the United States is dated from 1776, even though the British recognized it in 1783 (p. 3). He could have also been more generous in providing specific dates. (How many of us know John Adams's birthday?) An appendix listing the names and dates of American diplomats and commodores also would have been helpful.

Overall, Henson has written a very competent study, which specialists in Sino-Western relations will find useful.

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JOHN F. MARSZALEK. *Sherman's Other War: The General and the Civil War Press*. Memphis: Memphis State University Press. 1981. Pp. x, 230. \$14.95.

This interesting, too-brief book inquires into a large subject, aspects of the history of the First Amendment in wartime. John F. Marszalek, himself a military veteran, considers, after a skim of twentieth-century case law, tensions that grew between General Sherman and newsmen. Additionally, Marszalek offers interesting, if, to this reviewer, not altogether convincing psychobiographical suggestions about Sherman's rumored insanity (the general, he feels, was depressed, not insane) and need for independence from the Ewings. More substantial chapters deal with Sherman's military governorship, his exclusion of the press from the penultimate sweep eastward, and the deservedly aborted Sherman-Johnston "convention." All of which is a lot to pack into barely one hundred fifty pages.

Perhaps too much. On many points there is not enough to satisfy this greedy reader. As example, what did Sherman learn about constitutional relationships while he studied and practiced law, a question of particular importance in light of this

book's central concern (pp. 95, 123)? A book entitled, however hyperbolically, *Sherman's Other War*, cries for comparisons and contrasts of his press policies with those of other contemporary warriors here and abroad. How did Sherman's ways with newsmen mesh with Francis Lieber's General Order Number 100, with William Whiting's *War Powers*, or with the Duke of Wellington's distasteful but historically sound observation that martial law is largely the will of the commander? Marszalek himself suggests the last when he notes that Tecumseh's "anti-press attitude was based on personal biases rather than on a cogent philosophy of putting the constitution aside during the war" (p. 109). But only a few pages earlier Marszalek concludes, in language reminiscent of the *Milligan* opinion's illusory certitudes, that Sherman's arrest of a journalist because, allegedly, the general disliked the scribe and his tribe, "... was a clear violation of the First Amendment whether in peace or war" (p. 105). Similarly, Marszalek suggests that if a close subordinate of Sherman's had shot a reporter, as purportedly threatened, "Sherman would have been as guilty as if he had pulled the trigger himself" (p. 123). Would prevailing standards of criminal, military, or international law have reached out to Sherman? Not as I understand them.

Nor do I agree that the Civil War conscription and treason statutes were the only ones prohibiting or punishing disloyalty (p. 10). The several loyalty oath laws and orders, to some of which Marszalek himself refers, need to be added to this attenuated roster. And careless editing further mars this volume.

Yet Marszalek's book is useful and deserves employment. Sherman's rigid conservatism and uneducability about race equality are again verified (a point that would have justified an additional chapter on the general's post-Appomattox adventures with Andrew Johnson and the Washington press corps). Marszalek emphasizes soundly that before 1861 the states, not the nation, fettered freedoms, and, therefore, to both professional soldiers and reporters, the Bill of Rights had been largely irrelevant. General and journalists were woefully ignorant about each other's work in 1861. They were not much better off in 1865, or in 1917 for that matter. Prevailing case law was among the least fruitful sources for guidance. Like Americans generally, the military and the press were in a "no-precedents-land" during the Civil War. Yet, as *Sherman's Other War* suggests, they developed nevertheless acceptable, relatively unconstrained patterns of interaction; a remarkable achievement for a society caught up in a civil war involving race.

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ROBERT C. MORRIS. *Reading, 'Riting, and Reconstruction: The Education of Freedmen in the South, 1861–1870*. Chicago: University of Chicago Press. 1981. Pp. xv, 341. \$25.00.

Reading, 'Riting, and Reconstruction attempts to present a "comprehensive picture" of black education in the South from 1861 through 1870, the latter date being the final year of educational activity and support by the Freedmen's Bureau. In his initial chapter, Robert C. Morris surveys the religious and benevolent societies that became involved in freedmen's education, concentrating, as one would expect, on the American Missionary Association and the nonsectarian American Freedmen's Union Commission and their administrative personnel. One of the more informative sections of the chapter discusses creation of the Freedmen's Bureau in March 1865, and its subsequent involvement in education through Commissioner Oliver Otis Howard, a firm believer in government aid to education for freedmen.

Morris devotes three lengthy chapters to the teaching personnel of the black schools: Yankee schoolmarmes, black teachers, and Southern white teachers. A formidable amount of biographical information has been assembled for each category, but more analysis and generalization should have been attempted concerning the backgrounds, goals, problems, and achievements of these teachers and less effort made at "name-dropping." The inclusion of Francis and Archibald Grimké, for example, seems inappropriate in a study of black educators, although a *sine qua non* in a survey of famous black Americans.

The most noteworthy contribution of this monograph is Morris's chapter on the content of instruction in freedmen's schools, filling a void of previous publications. The author surveys the curriculum, textbooks, and methods of instruction used in the schools, pointing out that many texts were moderate in tone, especially the publications of the American Tract Society, which contained "extremely mild" references to slavery. Morris concludes that the use of these generally moderate materials, the imitation of the basic pattern of education in Northern schools, and the "educators' emphasis on order, morality, middle-class values and forgiveness obviously played a part in maintaining social stability in the post-emancipation South."

Informative chapter notes, ten excellent photographs and illustrations, and a (list) bibliography enhance the value of this work. Omitted from the notes and bibliography, however, are three secondary works whose use would have provided additional depth and insight: Donald Spivey, *Schooling for the New Slavery* (1978); Joe M. Richardson, *A History of*

Fisk University (1980); and Euline Williams Brock, "Black Political Leadership during Reconstruction" (Ph.D. dissertation, North Texas State University [1974]). The latter would have provided valuable information on a number of black educator-politicians, including Francis and Thomas Cardozo. These omissions notwithstanding, *Reading, 'Riting, and Reconstruction* is a valuable addition to the historiography of black studies, Reconstruction, and the history of American education.

WILLIAM PRESTON VAUGHN
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E. CULPEPPER CLARK. *Francis Warrington Dawson and the Politics of Restoration: South Carolina, 1874–1889*. University: University of Alabama Press. 1980. Pp. vi, 251. \$18.95.

Over the past decade the focus of efforts to understand the course of Southern history and American race relations has shifted from slavery and antebellum Southern society to the Civil War and the postbellum New South. Just as interpretations of the Old South turned on the central issue of the character and principal motivation of its ruling class, so has the examination of the New South sought first to define the character of the postbellum elite. With his generally sensible and sensitive biography of Francis Warrington Dawson, E. Culpepper Clark makes some useful contributions to the discussion of that era, though he falters at critical interpretative junctures.

The career of the English-born Dawson, who became an officer in the Confederate army, provides Clark a vantage from which to examine "the political economy of the Redeemer period." Dawson's alien origins did not prevent his becoming a social and political insider in postwar Charleston; indeed, the combination—of alien and insider—was probably crucial to his shrewd personal insights and bold political initiatives. Thus during Reconstruction he led the Charleston fusionists into a temporary alliance with the apostate Republican governor, Daniel H. Chamberlain; in the 1880s he briefly made common cause with the charlatan populist, Benjamin R. Tillman. As editor of the *Charleston News and Courier*, Dawson was "a Warwick" in South Carolina politics and rivaled Henry W. Grady as legitimate spokesman for the New South.

Clark rejects C. Vann Woodward's influential interpretation of the origins of the New South, "in part." Of course, Woodward describes a basic discontinuity in Southern history and social relations, points to the rise of a new ruling class, claims that these new men were allied with and subordinate to Northern and European capital, and exposes their

crass manipulation of both racism and Old South nostalgia to achieve class interests and objectives. Clark appears to differ mainly with the first of these arguments, insisting that the South Carolina "New Southerners" were really heirs of an 1850s generation that had mortgaged the state's future to make Charleston a gateway to the West. These men might be best described as "restorationists," thinks Clark, but here restoration is used as "a dynamic concept" rather than a reactionary one. What they sought to restore was not the old regime of planter aristocrats, but the bourgeois, commercial elite of antebellum Charleston. I suppose that there may be a significant difference between Clark's interpretation and Woodward's "Whig origins" argument, but it entirely escaped this reader.

The strength of the book lies in its subtle interweaving of Dawson's ideological orientation with his political activities and connections. Dawson slipped neatly into the nineteenth-century tradition of liberal reform. Like his Mugwump counterparts in the North, he believed in government by "the best men," favored civil service reform, general restriction of the suffrage, sound money, free trade, laissez faire, the rights of property, and the sanctity of contracts. Curiously, he flirted with radical opinion—the causes of labor, women, and Henry George; but "he always fell back on his traditional grounding in Smith and Ricardo." Like other New Southerners, he objected to grosser forms of racial control, but this was mostly finickiness about the form, not the substance of white rule. Dawson rejected any notions of a Herrenvolk democracy, however; only some whites were fit to rule, the unfit should be excluded from the franchise just as blacks were.

It is here that Clark—like so many others before him—loses his grip badly. He is dismayed that Dawson does not comment on race very much in his personal papers. Now one could surmise from this that matters of race might have been subordinate in his mind to those issues involving class power and control on which he had much to say. Instead, Clark chooses to infer a Myrdalian dilemma between Dawson's dual commitment to white supremacy and his "larger equalitarian faith." One is hard-pressed, however, to reconcile the "tortured mind" of this egalitarian Dawson with the shrewd manipulator who saw even independent white voters as a danger to elite hegemony.

Much more useful is the depiction of Dawson as simultaneously an apologist for the old regime and advocate of the right-forked, Eastern alliance. There is delicious irony in the fact that the controlling interest in the Charleston *News* was held by a New York capitalist. When forced to choose between the interests of the state and William P. Clyde's railroad syndicate, Dawson and the South Carolina elite chose the latter. Recently, collective

biographies, legislative roll call analysis, and tallies of stockholders in Southern industries have been invoked to support the contention that the old planters remained in control and the New South's ruling class was not really new. Among Clark's major contributions, therefore, are the indications emerging from his materials that the intersectional relationships are more subtle and the motivations more complex than such approaches can reveal.

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EDWARD L. GAMBILL. *Conservative Ordeal: Northern Democrats and Reconstruction, 1865–1868*. Ames: Iowa State University Press. 1981. Pp. viii, 188.

For some years there has been a need for a good, solid study of the actions and reactions of Northern Democrats during the early years of the controversial debate over Reconstruction. Edward L. Gambill has now provided us with that study. His modest-size, well-written, thoroughly researched book is a model of the kind that will increase and deepen our understanding of one of the most critical, complex periods in American political history.

The organizing theme of Gambill's study reminds us that in their preoccupation with Republican leaders and policies during the tangled process of nation-mending following the Civil War, historians have largely ignored the activities of Northern Democrats who "played an important role in the development of Radical reconstruction." Pursuing this theme, Gambill begins his study by focusing on the crippled state of the Democratic party in 1865: divided and defensive, devoid of strong leadership, weak at the polls, burdened by the wartime legacy of having opposed the Lincoln administration's handling of the war.

On the hopeful side, the end of the war did offer Democrats a chance to improve the fortunes of their party. Unlike their opponents, Democrats still functioned as members of a national party with strong support below the Mason-Dixon line. A defeated South, restored quickly to the Union, would help to transform that support into a revitalized party, providing the foundation for a grand coalition of Democrats strong enough to defeat a narrowly based Republicanism of faded sectional appeal. To build for the future in the face of decline also meant that Democrats would have to flesh out and unify behind new ideas and policies that would provide the party with an opportunity to regain political acclaim and hoist itself back into power.

The remaining chapters of Gambill's book deal with the reasons why none of this happened. The evolution of the bitter controversy over Reconstruc-

tion, particularly the issue of black suffrage, renewed and intensified the conflict between the pragmatists and ideologues in the ranks of the Democratic party. If Democrats had hoped to enter the postwar era with the purpose of once again regaining popular support, they certainly did not behave like a party whose disparate factions had agreed on a common strategy to back up that purpose. With artless egotism and considerable passion, Democrats spoke out in disagreement with each other on a curious mixture of conservative and liberal measures—from black suffrage and support for Andrew Johnson, to the terms for the South's readmission to the Union. The new teamwork some Democrats had worked to achieve floundered out of reach, tethered to old ideological commitments and a vivid disregard for the difference between the party's interest and personal advantage.

It is in these chapters that Gambill succeeds in giving us a more complex explanation of the role of Northern Democrats in the development of Radical Reconstruction. Thus, while Democrats were "essentially united" in their opposition to a Radical program, the nature of that opposition—noisy, confused, fragmented by factional feuds—had the effect, paradoxically, of pushing Republicans toward acceptance of a harsh Reconstruction program. Radical Reconstruction, in short, reflected more than a failure of Johnson's leadership. "For Democrats, it marked the bankruptcy of their strategy pursued since the end of the war."

To be sure, Johnson was no help. His leadership on Reconstruction amounted to an insubstantial policy that failed—a policy associated with uncertainty and mixed signals, with incompatible positions, with bungled decisions and clumsy overreactions—all of which made it impossible for Democrats to attach themselves too closely to his struggle with Republicans. Gambill is right in making this point. But the shortcomings and flawed judgments in Johnson's leadership resulted also from the self-serving activities of more than a few Democrats who encouraged him to cling to mistaken policies, while themselves trying to avoid becoming instruments of those policies.

Gambill's narrative is not only strong in general conception but also highly satisfying in its factual information. Certainly he has done very careful and thorough work in the primary sources, memoirs, and published documents. The alternation between accommodation and resistance among Democrats was a hallmark of their response to Reconstruction. Gambill's study has the virtue of presenting a detailed and convincing picture of the foundation, progress, and outcome of this development. He has written a splendid monograph.

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EUGENE H. BERWANGER. *The West and Reconstruction*. Urbana: University of Illinois Press. 1981. Pp. xiv, 294.

Here is grass-roots history at its best. Using extensive manuscript sources, Eugene H. Berwanger dispels the myths and romanticism of the American West during the post-Civil War period by a skillful and interesting recounting of the history of that region during the era of Reconstruction. Too long Reconstruction historians have emulated Hubert Howe Bancroft who showed little or no interest in the period. But now, we have a history that points out that nearly two million Westerners were intrinsically interested in the reforms and in the changing concept of federalism associated with Reconstruction. Since 72 percent of these people were not native and the majority were Northerners, it is understandable that they should have been concerned with the restoration of the Union. Although the social outlook was not liberal in 1860, increased repugnance to slavery and a belief that slavery had caused the war led to a favorable Western response to the Thirteenth Amendment. The assassination of Lincoln increased Western interest in Reconstruction. Although at first favorably disposed to Johnson, Western Republicans gradually abandoned him. Western Democrats momentarily embraced the president in the hope of securing federal patronage, but when this was not forthcoming they too forsook him. The break between Johnson and Congress in 1866 generally found Western Republicans siding with Congress, believing that the South would never agree to voluntary reconstruction and that the congressional program was moderate.

When the extension of the franchise to black men became a controversial Western issue, Congress passed the Territorial Suffrage Act of 1867 forbidding restricted suffrage and voiding all laws conflicting with this principle. In reality, this enfranchised only eight hundred blacks while some sixty-four hundred in five Western states were denied the vote. By 1870, however, black men were voting in every Western state and territory although their participation in politics was limited to the ballot. While Reconstruction issues revitalized the Democratic party in the West, as it did in the East, the Democrats were unable to revive the old states' rights principles over the growth of nationalism.

Western Republicans were reluctant to rush into impeachment proceedings against President Johnson and became increasingly unenthusiastic as the trial progressed. Berwanger believes that Senator Edmund G. Ross's decision to vote for the president's acquittal was motivated by his belief that the trial was a scheme by some Republicans to gain control of the nation through patronage. Interest in Reconstruction began to wane during the fall of 1867 while corruption and factionalism became the

two major political concerns of Westerners. Berwanger concludes that, despite its power struggle and emotionalism, the Republican party did bring about significant national humanitarian reforms and that Westerners played a role in this achievement. Included is an appendix recording the votes of Western representatives in Congress on Reconstruction measures, an extensive bibliography, and a most useful index. No student of Reconstruction history can afford to ignore this extremely fresh and well-researched book on the West and Reconstruction.

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JEROME A. GREENE. *Slim Buttes, 1876: An Episode of the Great Sioux War*. Norman: University of Oklahoma Press. Pp. xvi, 192. \$12.95.

Readers weary of the many trivial and often repetitive accounts of the Custer tragedy on the Little Big Horn will welcome this study of an important episode in the Sioux War of 1876. On balance the events were less spectacular and the participants certainly less flamboyant than the idealized cavalrymen whose demise at the hands of Sitting Bull and Crazy Horse on June 25, 1876, shocked the nation into disbelief. But because it provided concrete evidence of the government's determination to pursue a military solution to Sioux and Northern Cheyenne intransigency on the northern plains, the Battle of Slim Buttes less than three months later was enormously important in the larger setting of post-Civil War Indian-white relations.

For the forces of the Military Division of the Missouri under command of Lieutenant General Philip P. Sheridan, the situation in the summer of 1876 was anything but promising. The Indians had defeated Brigadier General George Crook on the Rosebud, obliterated Custer's force at the Little Big Horn, and repulsed Major Marcus A. Reno's command nearby. Under these circumstances Crook and other field commanders on the Yellowstone were ordered to pursue and subjugate the Indians in one of the most bleak and inhospitable regions of the entire West.

What follows, in Jerome A. Greene's narrative, is an analysis of Crook's maneuvers culminating in the seizure of a strategic Sioux village at Slim Buttes, near the south fork of the Grand River due north of the Black Hills. The forced march, often called the "Horsemeat March" because Crook's decision to move with extremely light provisions led to his men being forced to abandon some and then subsist on the remainder of their somewhat less than palatable mounts, has been chronicled elsewhere—but not with the expertise and thoughtful criticism found

here. The fact is, as the author rightfully emphasizes, Captain Anson Mills's attack at Slim Buttes on September 8 was as much motivated by the need for food and supplies as it was to force American Horse and his people into submission. Greene's well-documented and dispassionate treatment of internal bickering and enlisted-man criticism of Crook's tactics, as well as a good deal of luck in timing and tribal position vis-à-vis the army, mark this study as one of the more sophisticated books on the Sioux War of 1876.

WILLIAM E. UNRAU
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MARTIN V. MELOSI. *Garbage in the Cities: Refuse, Reform, and the Environment, 1880–1980*. (Environmental History Series, number 4.) College Station: Texas A&M University Press. 1981. Pp. xvi, 268.

This book with the not so appealing title is a sound scholarly study of an important, and neglected, subject. Urban civilization has always been rich in material remnants, but the disposal of solid waste has been a particularly severe challenge in modern industrial societies. Martin V. Melosi looks directly at this challenge and traces its dimensions, and the urban responses to it, from the nineteenth century to the modern environmental movement.

In the nineteenth century, garbage was omnipresent in urban America but seldom thought about—until it was associated with disease. Even then, collection methods were primitive and seldom sanitary. As early as the 1880s, New York City dumped so much refuse into the Atlantic "that the approaches to the harbor were often clogged, and the public and private beaches on the New Jersey shoreline looked like cesspools" (p. 42).

Colonel George E. Waring, Jr., street-cleaning commissioner of New York from 1895 to 1898, is the acknowledged father of modern sanitary engineering in the United States, partly because of his flamboyance but mostly because he brought together many different techniques and reforms and linked these to a broader public concern for the urban environment. Waring was very much in the "progressive" mainstream, and Melosi's description of his life and work is one of the best features of the book.

In the early twentieth century, sanitary engineers emerged as the technocrats charged with solving problems in the urban physical environment. They adopted the germ theory of disease and advanced their cause through professionalization and the increasing municipal control of street cleaning, water supply, and waste collection and disposal. Municipal reform organizations—especially women's civic groups—prompted a heightened public awareness

of the necessity for good urban "housekeeping," for aesthetic as well as health reasons. These efforts were never effectively combined, but they did result in much improved street-cleaning methods and more effective means of garbage disposal of which incineration was the most preferred if not the most prevalent.

But the generation of waste in urban America always seemed to outstrip new ways of dealing with it. According to one estimate, "solid waste increased about five times as rapidly as population" from 1920 to 1970 (p. 192), and it contained more paper products and plastics, the marks of an affluent consumer economy. Nowadays, Melosi reports, the \$4 billion annual cost for waste collection and disposal ranks only behind schools and roads as a major local expenditure.

The largely elitist character of this reform activity is well described, and Melosi sets it in a broader reform context, including the "City Beautiful" movement and emphases on economy and efficiency. More could have been said about the specific composition of municipal reform groups and especially about the impact of the particular dynamics of professionalization on sanitary engineering. The inequitable distribution of this public service among various urban areas and populations should also have been more adequately addressed.

On the whole, this is a very solid monograph that should guide and enlighten future research in an important dimension of the urban experience.

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MARTHA MOORE TRESCOTT. *The Rise of the American Electrochemicals Industry, 1880-1910: Studies in the American Technological Environment*. (Contributions in Economics and Economic History, number 38.) Westport, Conn.: Greenwood Press. 1981. Pp. xxxviii, 391.

The chemical industries in America have been examined by numerous historians, but comparatively little attention has been paid by them to electrochemicals. *The Rise of the American Electrochemicals Industry* rectifies this scholarly omission. Moreover, none of the other American chemical industries has been treated in the comprehensive, multifaceted fashion in which Martha Moore Trescott treats electrochemicals. In a series of well-written and well-integrated studies she illuminates this corner of the "American technological environment," as she aptly terms it.

Traditional histories of a particular industry have

generally concentrated upon the internal, heavily technical dynamics of specific businesses. Trescott has not ignored this dimension of electrochemicals and has in fact investigated over seventy firms. Influenced, however, by the pioneering writings of David Landes and Alfred Chandler, among others, she has probed no less deeply into the roles of management, of research laboratories, of universities, of information networks, and, not least, of women in describing American electrochemicals' formative stages. She demonstrates that such developments as distinctive American machine shop practices, metallurgical expertise, industrial work arrangements, chemical engineering instruction, and corporate research—plus the unacknowledged contributions of female electrochemists and relatives of male inventors and entrepreneurs—were as critical to electrochemicals' growth as the tools and machines so often studied in virtual isolation from nonmaterial factors.

One of Trescott's principal points is the transition within electrochemicals from single *products* to complex *processes* (with many products) as part of the overall maturation of America's chemical industries. So diversified, indeed, did electrochemicals become that she concedes the difficulty of conceiving it as just one industry. But she successfully surmounts that problem in the course of her book.

Another of Trescott's principal points is that electrochemicals' history antedates the establishment of the Niagara Falls hydroelectric system in 1895, the industry's conventional starting date. Not only did the industry exist elsewhere in America as far back as 1880, but even then it existed outside the United States as well. Thus World War I did not in itself create the electrochemicals industry, as commonly assumed, but rather was fought partly with the industry's recent technical advances.

Trescott's final principal point is that the extent to which the American electrochemicals industry differed from its European counterparts reflected a distinctive American "technical culture" (p. xxxiii). That culture, merely outlined above, promoted the industry to international stature and economic power long before World War I. She promises further research into how this single industry "helped crystallize [all] our modern chemical industries" after 1910 (p. xviii).

The Rise of the American Electrochemicals Industry represents prodigious and painstaking research and writing. As Trescott herself states, it also represents "an intersection of the history of technology, business history, the history of science, and economic history" (p. xvii). Her ability to synthesize those four specialties is a not inconsiderable achievement.

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GERALD G. EGGERT. *Steelmasters and Labor Reform, 1886–1923*. Pittsburgh: University of Pittsburgh Press, with the cooperation of the Pennsylvania Historical and Museum Commission. 1981. Pp. xvii, 212. \$17.95.

In this volume, Gerald G. Eggert examines the views and policies of management in the steel industry concerning labor relations. The focus is on the work of William Dickson and his efforts to convert others to his positions. We do learn a good deal about labor policy from the management side, and thus the book nicely complements David Brody's work on the steelworkers. Eggert's approach, however, runs into a basic problem: although Dickson was a major spokesman in the steel industry on labor questions, his influence on policy was limited. Thus we find out more about Dickson's views—which fit into the liberal management position of the period, with its emphasis on welfare capitalism, company unionism, and reasonable working conditions—than we do about the motives and actions of those in management who actually made the decisions. These men often differed sharply with Dickson. In addition, those portions of the steel industry with which Dickson was not in direct contact are not discussed very fully. Thus the book is less comprehensive than it might have been.

Eggert offers his study as an example of the institutional transformation that marked the period between 1890 and World War I. In exploring these developments in the steel industry, he believes the human factor was significant in comparison with basic technological and economic forces. Thus there were alternatives, and control of management by other leaders—for example, Dickson—would have produced markedly different results. This is certainly true for labor policy, but Eggert fails to sustain the argument that a more competitive structure for the industry was feasible with different leadership. Beginning with the integration of manufacturing in Carnegie's time, through the formation of United States Steel in 1901, the tendency toward an essentially noncompetitive industry seemed irreversible. In fact, when United States Steel reluctantly and briefly adopted more competitive practices in 1909 and 1921, it seemed clear that the giant company could control and even destroy other firms in the industry.

Beyond these limitations, there is much valuable material. The volume forcefully reminds us that the history of American workers must include the nonorganized in the basic industries. Eggert provides keen analysis of why Dickson and, to a lesser degree, others in management chose particular policies in labor matters. He properly places labor relations within management's broader positions. Eggert also points out the importance of the Pro-

gressive reformers as a pressure group. With skill, he indicates not only the influence they had, as United States Steel feared antitrust action or laws to control hours, but also the limits of the reformers' influence when suggested changes forced management to choose between profits and good public relations. Another positive feature of the book is the clear discussion of welfare capitalism and the reasons why steel companies turned to it. The deterrence of unionism, the avoidance of accident liability suits, and the increased dependence of the worker upon the company emerge as the basic factors.

In sum, the book is more limited in scope than the title suggests, but within these boundaries Eggert provides much of value for students of both labor and business history.

IRWIN YELLOWITZ
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MICHAEL BEZILLA. *Engineering Education at Penn State: A Century in the Land-Grant Tradition*. University Park: Pennsylvania State University Press. 1981. Pp. viii, 239. \$16.95.

DAVID C. SMITH. *The Maine Agricultural Experiment Station: A Bountiful Alliance of Science and Husbandry*. Orono: Maine Life Sciences and Agriculture Experiment Station, University of Maine at Orono. 1980. Pp. xiv, 292. \$10.00.

Each of these two slim volumes is a significant contribution to the educational, institutional, or scientific history of the United States. David C. Smith's book opens with an account of the establishment in 1885 of what is now known as the Maine Life Sciences and Agriculture Experiment Station at Maine State College, later the University of Maine, Orono. The early role of the station in inspection work and in the setting of agricultural standards is discussed. Sometimes such efforts took the form of playful cajolery by station scientists. Raymond Pearl, for example, cautioned poultrymen in 1912 that "chickens are not machines. . . . A poultry plant is not a factory. It partakes more of the nature of a girl's boarding school with a strong leaning on the part of its inhabitants toward suffragette doctrine" (p. 62). There are numerous references to other outstanding scientists at the station, and to their achievements, such as Edith M. Patch, a leading authority on aphids and the first woman to be elected (1930) president of the Entomological Society of America, and Geddes W. Simpson, whose work changed the potato business in Maine and elsewhere.

Following the outlining of the struggles of the station through the financially traumatic period

from 1921 to about World War II, the rise of the influential modern station is told, with its ever-increasing emphasis upon basic research and its progress in the areas of forestry, microbiology, and agricultural economics. Such influence is attested by the fact that in the 1970s two consecutive directors of the station were successively named vice-president for research of the University of Maine.

Michael Bezilla's volume reflects the author's belief that the importance of engineering education in the history of Pennsylvania State University has been consistently overlooked and neglected, by its alumni as well as its historians. He ascribes this neglect, in part, to the predominantly agricultural context of Penn State's founding and early operation. Originally chartered by the state legislature in the 1850s as the Farmers' High School and located in a rural area in the center of the state, it soon became the Agricultural College of Pennsylvania. Despite a later change of name (1874) to Pennsylvania State College, it was not until the 1890s that the "ag or cow college" image was broken. This was largely brought about during that decade under the aegis of President George W. Atherton (1882–1906), who was instrumental in setting up a school of engineering along with six other schools, which effectively established the administrative and academic structure of the college. Presided over by Dean Louis E. Reber (1895–1907) and John Price Jackson (1907–14), the engineering school experienced a significant expansion in physical facilities, increased enrollments, and the development of graduate and extension education. The tenure of Jackson's successor, however, Dean Robert L. Sackett (1915–37), ushered in by a disastrous fire at the engineering school and concluded with the Great Depression, was characterized by stagnant enrollments. The World War II period saw the augmenting of the physical facilities at the school and a broadening and revitalizing of its curriculum under Dean Harry P. Hammond (1937–51). Bezilla then describes the activities of the two men who largely shaped modern Penn State. The first, Eric Walker, came in 1945 as chairman of the engineering school's department of electrical engineering where he remained until 1951 when he succeeded Hammond as dean. The second, Milton S. Eisenhower, brother of President Dwight D. Eisenhower, became president of the institution in the same year and in 1953 had its name changed to Pennsylvania State University. In 1956 he named Walker vice-president for research. Eisenhower resigned as president in the same year and was instrumental in having Walker named his successor. Their years at Penn State (Walker retired as president in 1970) saw a vast increase in enrollment in undergraduate and graduate engineering in all fields. They were also marked by a great surge in the volume and variety

of engineering research, much of it centering on the then-emerging nuclear and computer fields. Under Walker's successors as dean, Merritt A. Williamson (1956–66), Nunzio J. Palladino (1966–81), and Wilbur L. Meier (1981–), engineering education has continued to play a major role in Penn State's total program.

Each of the authors of these two volumes correctly points out that his history covers topics that have received little formal study by U.S. historians. There are other points of similarity; both works were commissioned and sponsored by officials of the entities they study. This proved to be a plus in both cases in that it ensured access to the documentary materials and the papers and reminiscences of knowledgeable individuals upon which the histories are based. Old photographs are used to good advantage throughout both volumes. Both also contain excellent appendixes that provide statistical information that a reader can turn to in charting his way through a century of institutional change and development. Finally, both accounts are readable, that is, there is a blending of men and events that prevents bogging down, which is a characteristic of some educational or institutional histories. This reviewer found both books informative and interesting, and they should serve to encourage other historians to undertake similar and needed histories in other states and regions of the United States.

JOSEPH C. KIGER

University of Mississippi

RONALD C. TOBEY. *Saving the Prairies: The Life Cycle of the Founding School of American Plant Ecology, 1895–1955*. Berkeley and Los Angeles: University of California Press. 1981. Pp. x, 315. \$25.00.

This book provides a thoughtful and thorough history of the school of grasslands ecology founded in the late 1890s upon the plant succession theories of Frederic Clements. It would be valuable for that alone, for Clements's theories and followers dominated American plant ecology for a half-century. But Ronald C. Tobey offers much more than an account of the rise and fall of a scientific idea and the scientists associated with it. Tobey uses the story of the grasslands community to test contemporary sociological theories about the structure and functioning of scientific communities and to raise, if not to settle, questions about science as a social activity and its relation to the rest of our culture. This is an ambitious program, but it is well planned and executed. *Saving the Prairies* should be of interest to a variety of scholars: environmental and agricultural historians and historians and sociologists of science.

Despite his declaration that he is not writing a "straightforward history of ideas," Tobey empha-

sizes intellectual developments. He traces the traditions of European plant geography, which formed the background to the work done at the University of Nebraska in the late 1890s, and discusses at length the ideas that influenced Charles Bessey, the founder of the school and mentor of Frederic Clements. He reconstructs in detail the epistemological and technical innovations that led Clements and Pound, working on the phytogeography of Nebraska in 1896, to their crucial insights and traces in detail the progress of the theory. There is a full chapter on the career and changing ideas of the most visible European critic of Clementsianism, A. G. Tansley, and another on the changes in ideas forced upon the grasslands ecologists by the drought of the 1930s.

What distinguishes this from what Tobey sees as outmoded intellectual history is the attention paid to the social and sociological context. Tobey takes pains to recreate the atmosphere of the "cow college" in which grasslands ecology developed, to show how the lives and experiences of the ecologists shaped their ideas (Clements, for instance, thought direct experience of the plains crucial to understanding and appreciating the theory), and to show how changes in society affected their research goals and practices. He also (and this is an unusual feature for a historical work) builds the narrative upon an explicit theory of social change and scientific development—here the ideas of Thomas Kuhn about the importance of paradigms as a guide to "normal science" and Diana Crane's model, from *Invisible Colleges*, of the way in which scientific schools form and break up. This double development, of a historical narrative and of the narrative as a test for a theory, is one of the most interesting and valuable features of the book. It makes explicit what too many of us only implicitly rely on—a general theory of which our own case is but a particular example—and it provides a means of examining (and possibly grounding in reality) social science concepts.

Tobey's attempt to show how the "content of the grassland community's knowledge was intimately related to the social structure of the community and to the role the community played in American society" (p. 5) is the most difficult, ambitious, and, to my mind, least successful part of the book. It is not that Tobey has failed, only that there exist more grounds for argument here than in other areas. It is hard, for example, to see how the technical or scientific innovations of Clements and Pound or Clements's finished theory were connected to the practical concerns of the University of Nebraska or to the problems of Nebraska farmers. Indeed, the development of the grasslands school seems a good lesson in the ways in which scientists in cow colleges managed to preserve a more traditional and less "practical" orientation than their clientele demand-

ed. Other aspects of the connections he makes could be criticized, but it is hardly worthwhile. The problem is simply too complex to be adequately treated in a volume in which the author must develop the full background to his case; one can only hope that Tobey will continue work on the question of the influence of society on scientific ideas.

Tobey has, as a final stretch of his research program, done some serious quantitative work, but while the effort was laudable the results do not justify the labor. We do not need charts and tables to know that Clements's *Plant Succession* (1916) was a key document in the development of grasslands ecology, nor elaborate study of citations to work out the clustering of scientists, nor an analysis of career patterns to discover that University of Nebraska PhDs had different lives than those from the University of Chicago. The work provides precise answers, but precise answers to imprecise questions.

These caveats should not obscure my admiration. *Saving the Prairies* tried to do many things and manages all of them at least competently and some of them brilliantly. It deserves far more analysis and thought than any individual reviewer or journal can give it and is certain to be one of those books that will repay rereading a few years hence, when we shall have done enough research to grapple with the questions this pioneer work has raised. Historians of science and environmental historians, in particular, are in Tobey's debt for this excellent work.

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State University

ALEXANDRA OLESON and JOHN VOSS, editors. *The Organization of Knowledge in Modern America, 1860–1920*. Baltimore: Johns Hopkins University Press. 1979. Pp. xxi, 478. \$22.95.

These seventeen richly informative and pathmarking essays demonstrate that it is indeed possible for many scholars to pursue a significant historical theme successfully in one volume. Here the theme is the growth, organization, specialization, institutionalization, and professionalization of *formal learning* (I think a more accurate term than "knowledge") in the American half-century after 1870. Until some of the topics receive lengthier treatment, this book will undoubtedly be a standard reference work. Therefore in a journal of record I am obliged chiefly to list authors and subjects. All these essayists exemplify the attitude and techniques (and the limitations) of intense empirical inquiry whose origins they trace in the salad days of Germanic scholarship in the United States. Many emergent and some older disciplines are caught up for inspection. Seven essays support John Higham's opening

discussion of scholarly and scientific specialization in a democratic society. The two most important of these for historians may be Laurence Veysey's on the "plural organized worlds of the humanities" and Dorothy Ross's briefer essay on the social sciences. Although Veysey's organization is unavoidably clouded by his admirable determination to see the humanities as vague and somewhat artificial academic entities, often subject to "unguided drift," his staggeringly impressive endnotes reinforce the thesis of complexity and assure us how fresh and inviting are the sources he mines. Ross underscores the crisis in values among both social scientists and humanists of that era, torn between the intellectual authority of a village and religious past and that of modern, scientific opportunities. Yet the "scientistic orientation" of the social scientists, she adds, at least gave them an intellectually unifying perception ironically lacking among humanists who rarely were pluralists. Indeed the next two essays illustrate that very aim of scientific confidence and purpose that was frequently missing from earlier humanistic inquiry. Daniel Kevles, with some remarkable career tabulations, traces the "communities" of physicists, mathematicians, and chemists from 1870 to 1915. Garland Allen's biographical essay on Thomas Hunt Morgan becomes a tour de force as an abbreviated history of transformed biology to about 1930. Concluding this section on specialization are essays by Margaret W. Rossiter on the organization of the agricultural sciences, John Rae on the application of science to industry, and by Louis Galambos on the American economy and the reorganization of the sources of knowledge, wherein he emphasizes that corporate leaders before 1920 were slow to exploit the new sources of formal knowledge available to them.

The second section of the book concerns the institutional context of learning. It is introduced and encompassed by Edward Shils's brilliant essay on the importance of the university to "the order of learning" in the United States. In this memorable chapter Shils delineates the moral endeavor (once a pious custodianship) that was scholarship growing within academic communities, especially within the dominant "central constellation" of empirically minded research universities, spurred on by departmental specialization. Accompanying Shils, Hugh Hawkins grapples successfully with the issue of the dual teaching-research function in the university age, arguing that this very ambiguous and tension-ridden duality, together with a guarantee of transgenerational scholarly continuity, gave unique importance and support to universities in a utilitarian society. Case studies completing this section are brief, fresh, and revealing: Nathan Reingold discusses a private research organization, the Carnegie Institution of Washington; A. Hunter Dupree and

Sally Gregory Kohlstedt write on a national and a regional learned society, the National Academy of Sciences and the Boston Society of Natural History (now the Boston Museum of Science). The changing functions of public and private libraries in an increasing "age of use" are well depicted by John Y. Cole.

In a final section Fritz K. Ringer emphasizes that the path of German scholarship, though parallel to the American, had essentially different institutional manifestations by virtue of contrasting social and cultural origins. Neil Harris reminds us that in this period the lamp of learning was also oiled by popular institutions, encyclopedias, avocational and patriotic societies, and charlatans. Lastly, addressing himself to "an ecology of knowledge," Charles Rosenberg challenges historians to inquire further into the complex and often inconsistent process by which social needs and values interact upon areas of knowledge, a process wherein professional norms, disciplinary ideas, and institutional aims are pursued.

Notwithstanding limited space, I must add that a critical essay on this book would surely confront some disturbing aspects of the sociology of American knowledge down to 1920: the creeping Mandarinism and filiopietism of the academic man, the nationalization of academic purpose, the limited aesthetic and moral vision of learned people intensified a century ago. If these traits have in some ways become increasingly evident, the essays in this volume will help to explain the origins of our academic mentalité.

WILSON SMITH
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Davis

THOMAS J. OSBORNE. *"Empire Can Wait": American Opposition to Hawaiian Annexation, 1893-1898*. Kent, Ohio: Kent State University Press. 1981. Pp. xv, 180. \$18.00.

Americans began arriving in the Hawaiian Islands in the 1790s. As early as 1842 President Tyler proclaimed them off-limits to other nations, and a decade later talk began of annexing them to the United States. Yet not until 1898 did the union take place.

Thomas J. Osborne's small book argues that the main reason for delay was the persistent opposition of American anti-imperialists. From February 1893, when lame duck President Benjamin Harrison submitted a treaty of annexation to the Senate, to the summer of 1898, when Congress finally approved annexation by joint resolution, opponents held off the expansionists. Resisting were ideological anti-imperialists as well as such other anti-annexationists

as sugar growers and xenophobic labor unions, supported in their efforts by President Cleveland and his secretary of state, Walter Q. Gresham. Osborne argues that the most important motives of the anti-imperialists were ethical, historical, racial, and constitutional. They believed that acquiring far-off islands, especially without the natives' consent, unwisely broke with American traditions and would result in future orgies of annexation, in militarism, and in political degradation. Osborne wrongly claims originality in contending that the debate in 1893–94 over Hawaii anticipated the key issues of the “great debate” of 1898–1900. Contrary to Walter LaFeber and Thomas McCormick, he insists that the anti-imperialists differed fundamentally from the annexationists: they stood on principle and, though not opposed to commercial expansion, were not “trade chauvinists” (p. 119). Finally, he argues that annexation finally occurred in 1898, not for military reasons, but because control of Hawaii, linked to the U.S. presence in the Philippines, was seen as an essential part of the path to the China market.

Osborne's most useful contributions are the detailed, if conventional, description of the first Hawaii debate in 1893–94; the careful tracing of the evolution of the Cleveland administration's Hawaiian policy, concluding in a feckless abandonment of the issue to Congress; and the rich discussion of the reprise on Hawaii in 1897 and 1898. Although these chapters make no attempt at serious roll call analysis and are peppered with undemonstrated assertions about public opinion and the relative importance of various viewpoints, they also rest on extensive research and offer both unfamiliar material and fresh insights.

Unfortunately, though the size of the book is modest, its tone is not. Specialists will have noted already that virtually nothing in the theses summarized is surprising. The book is useful and often stimulating primarily because Osborne has filled in important gaps in the narrative. He nearly spoils the effect, however, by his erection of straw men (for example the constantly repeated insistence that most historians think Hawaii was annexed for military reasons), his hair-splitting claim to historiographical uniqueness (namely, that no one else has ever written a “monograph” solely about opposition to Hawaiian annexation), and his other unfounded claims of originality (for example, that virtually all historians of anti-imperialism believe the “debate” over imperialism began abruptly in the autumn of 1898). Perhaps the most ungracious example is his statement that, though LaFeber and McCormick preceded him in demonstrating the connection between Hawaii's annexation and the China market, they cannot be credited because their interpretations were based on less information than his,

because they provided less “close argumentation” (p. 126) than he, and, worst of all, because they did not expressly refute the “military exigency” argument.

These departures from civility aside, this book provides abundant new narrative material that supports the interpretations of both the author and other historians who, together, continue to expand our understanding of Hawaii's place in the expansionism of the 1890s.

ROBERT L. BEISNER
American University

EDNA BONACICH and JOHN MODELL. *The Economic Basis of Ethnic Solidarity: Small Business in the Japanese-American Community*. Berkeley and Los Angeles: University of California Press. 1980. Pp. x, 290. \$18.95.

Scholars have presented competing paradigms and models to explain the status and role of the Japanese in America. Unlike the assimilation or cultural pluralistic models, which attempt to integrate the total experiences of this group, this volume presents a limited perspective based on economic behavior. Moreover, Edna Bonacich and John Modell's intent is to develop a particular theoretical perspective, the “Middleman Minority” thesis, and to explore its application to the Japanese-Americans.

This book is divisible into two parts: the first section develops the Middleman concept that is then applied to the Issei, or immigrant generation, by a reinterpretation of standard secondary source materials. The second section focuses upon the Nisei generation by interpreting selected data derived from a recent sociologically oriented nationwide survey of Japanese-Americans.

There are some problems with this work. The Middleman Minority concept is unclear; even the authors admit to this, saying, “Despite the lack of precise conceptualization . . .” (p. 14). Moreover, their definition does not neatly fit the Japanese-American experience. The Japanese-Americans have not previously been seriously viewed as middlemen because the theory was developed for an analysis of relations in quite different kinds of socioeconomic conditions than existed on the Pacific Coast in the early 1900s. The Middleman Minority concept usually refers to a group that serves a distinct and significant economic function within a society, between its elites and its masses who lack the background for providing the function themselves. The theory assumes an elite and masses stratification, but no such stratification existed on the West Coast. And the authors develop no argument that West Coast conditions were basically the same as for the Jewish traders in Europe and the Chinese in Indonesia or Africa. There is also the implication in

the Middleman theory that the group in question exploits the society by making itself indispensable and also keeps itself aloof from the society while exploiting it. There are better ways to explain why the Japanese-American communities remained segregated than the Middleman Minority theory can offer.

The authors would like to recast the Japanese-American experience in the Middleman Minority crucible. To do so, the concept is redefined to mean an ethnic group that concentrates its activities in the trades or small enterprises, including small farms. So defined, the significance of the concept for ethnic relations theories becomes ambiguous. For example, the Japanese minority was not unique among groups on the West Coast entering into small enterprises; by the authors' definition though, all other groups fitting into this category would have to be called Middleman Minorities. Moreover, the authors' contention that the agricultural area, where many Issei found their livelihood, can be redefined as a Middleman trade or business is unconvincing: the Issei role in the fields was as a producer-seller, not buyer-seller. And those who opened small businesses in urban areas catered usually to immigrant Japanese needs. Further, the authors state that the Issei were interested in their sons remaining in the ethnic economy and that the Nisei really wanted to stay there. This contention fits the Middleman hypothesis but supporting evidence is scanty. Most other sources argue that the Issei desired to have their children gain a better livelihood, regardless of location, and that the Nisei preferred to leave the ethnic enclaves for their own economic betterment. The high percentage of Nisei who went to college, majoring in such fields as engineering, education, architecture, and foreign trade, indicates that there was less interest in remaining in small businesses within their parents' trades.

The Japanese-Americans have been and will continue to be the focal point for varying interpretive studies. An economic or class analysis of the Japanese minority could be made plausible, although there would be points of inconsistency, but the Middleman Minority thesis does not capture the essence of the many-faceted experiences of this group.

TETSUDEN KASHIMA
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JAMES HENNESEY. *American Catholics: A History of the Roman Catholic Community in the United States*. Foreword by JOHN TRACY ELLIS. New York: Oxford University Press. 1981. Pp. xvi, 397. \$19.95.

James Hennesey attempted to write a broad-ranging history of the American Roman Catholic com-

munity. He fulfilled his task admirably. He presents an encyclopedic knowledge synthesized and interpreted so that the trees never obscure the forest. This book can be read with value by either layman or scholar. It is a thorough success.

The book is divided into three parts. The first hundred pages, almost one-third, describe and analyze the "basic 'American period,'" from 1634 to 1829. During these years relatively few Catholics settled in what is now called the United States. If Hennesey had devoted less space to the early period, having few Catholics, he could have more fully examined the "immigrant period," the nineteenth and first half of the twentieth century. During these years Catholics increased in number from approximately three hundred eighteen thousand to over twenty million. Moreover, it is surprising that in a book that the author describes as a "peoples history" there was so little discussion of the social conditions of ordinary working-class Catholics. There is little flavor of the massive poverty faced by Irish-Americans, and of the wretched urban slums from which they rose. In a sense, the Irish conquered the slums, partly through nonclerical leaders—the children and grandchildren of the Irish poor. Only one paragraph is devoted to the late nineteenth-century Catholic lay political leadership. Although urbanists have produced considerable scholarship in the last decade pointing out the efficacy, and advantages that "boss" politics added to city life, Hennesey mentions only the negative aspects, which undoubtedly existed. The same chapter had more than twice the amount of material devoted to Catholic missionary activity in the Rocky Mountains than was devoted to Catholic laymen (often Irish) that governed the major cities of the nation facing explosive urban growth. In general the lay Irish have not been given enough attention.

Polish, Lithuanian, Croatian, Italian, and other new immigrant groups could have been further explored as well. Hennesey, on the other hand, provides considerable information on other Catholic ethnic groups that are rarely explored in general Catholic histories. He discusses black Catholics in relation to civil rights, missionary work, slavery, lay societies, and as clerics. Likewise, native American Indians and Latins are similarly examined.

The last chapters deal with twentieth-century developments, both as an aspect of the immigrant experience and from the perspective of the 1960s, the latter being a time of turbulence within, and outside of, the church. The author's examination of Father Charles Coughlin and Joseph McCarthy are particularly thorough. Hennesey does not gloss over what might be considered unpleasant detail. It continues to be refreshing that Catholic scholars produce objective monographs, particularly at a time when certain other religious institutions exert

pressure upon their academic constituents to produce a literature that is theologically "correct."

On the whole, this objective and highly professional work by one of the major historians within the church is a solid contribution to American Catholic historiography.

NEIL BETTEN
Florida State University

CHARLES SHANABRUCH. *Chicago's Catholics: The Evolution of an American Identity*. (Notre Dame Studies in American Catholicism, number 4.) Notre Dame: University of Notre Dame Press. 1981. Pp. xi, 296. \$18.95.

Stimulated by broad interest in the sources of contemporary social problems, American historical scholarship has provided in the last generation a vast literature on the themes of industrialization, urbanization, and the Americanization of immigrants. One result has been a new interest in the Catholic church, the church of the immigrants so obviously present in every industrializing center, its parishes, schools, and voluntary societies—major, if not dominant elements in the lives of masses of factory workers—its symbols and beliefs central to the consciousness of strategically situated populations. Church historians themselves, having mined the ecclesiastical records of the nineteenth century, saw the need for such a social history of American Catholicism, which would locate the American Catholic experience in the concrete situations of American life. The convergence of these streams of interest is generating a flourishing field of American Catholic studies represented in part by the "Notre Dame Studies in American Catholicism," of which *Chicago's Catholics* is the fourth publication.

Charles Shanabruch examines the organized Catholic response to ethnic diversity in the nation's most cosmopolitan and eventually largest diocese. In Chicago, as elsewhere, the task of church leadership was to preserve the faith and loyalty of successive groups of immigrants, "melding these miscellaneous peoples into a organized Catholic body," while at the same time winning for the church a secure and respected place in a not always receptive society. After recounting the chaotic early history of Chicago Catholicism, whose first missionary bishops were overwhelmed by ethnic rivalries, Shanabruch demonstrates how three successive bishops, Patrick Feehan, James Quigley, and George Mundelein, guided the rapidly growing Chicago church through the twin obstacles of internal diversity and external hostility. First, the quiet, retiring Feehan fostered the growth of national parishes, developed a network of supportive priest-advisors representative of the church's diversity, and

skillfully used national loyalty to build "a confederation of immigrant parishes and institutions." Internal rivalries were overcome by accommodating the demands of each major group for parishes and schools. Feehan's "loosely run, pluralistic administration, one that did not espouse a particular nationalistic conception of religious behavior," provided space for popular initiatives and made few demands on particular Catholic groups.

At the same time, Catholics were gradually brought to a sense of unity, a "supra national Catholicism," by the shared participation in resisting and eventually overturning the Edwards law, which limited their right to maintain independent schools. Quigley, succeeding to the see in 1903, continued this process, giving careful attention to each group while cementing "the coalition of nationalities" through clerical discipline, emphasis on the church as a conservative, constructive force in the city, expansion of charitable services, opposition to radicalism and middle-class progressivism, and support for labor unions and bread-and-butter social reforms. It remained for George Mundelein to take the next step to a more fully American Catholic identity. Building on the expanding population of native-born Catholics and priests, Mundelein limited parochial autonomy, ended the building of new national parishes, established a diocesan school board, with common texts, and a diocesan charities office for centralized funding of social services. With a "near mania for order," Mundelein built a more organized, episcopal-centered church, which emphasized unity achieved through a clerical chain of command, while championing the conservative values required by democracy. Loyalty to America, through the church, in bond drives and good citizenship campaigns, now took precedence over the cosmopolitan, ethnocentric, parochial Catholicism of the earlier generation. Internal diversity remained, circumscribed by tighter clerical discipline and increased episcopal power centered in growing bureaucracy, while Catholic separatism persisted, strengthened by the cultural conflicts of the twenties. Out of the internal dynamics of unity and diversity and the external conflicts with recurrent waves of nativism, the bishops, by emphasizing organizational imperatives, had built a church that while "never monolithic, but one from many . . . moved toward being one with America while remaining one with Rome."

By carefully studying the organizational response of the church to urban growth and expansion, Shanabruch has made an important contribution to America urban and social history as well as to Catholic studies. His work corrects some earlier judgments about the impact of nativism, the interrelation of ethnic groups, and the means by which bishops controlled their diverse flocks, while it puts

flesh on the term Americanization. Filled with information, attentive to trends beyond Chicago, and skillfully written and argued, this book deserves a wide audience.

DAVID J. O'BRIEN
College of the Holy Cross

JOSEPH JOHN PAROT. *Polish Catholics in Chicago, 1850–1920: A Religious History*. DeKalb: Northern Illinois University Press. Pp. xvii, 298. \$10.00.

Although perhaps too detailed to hold the interest of the general reader, *Polish Catholics in Chicago, 1850–1920* by Joseph John Parot offers the specialist an incisive glimpse at the inner workings of the immigrant church. Parot's temperately told, sympathetic account weaves together two stories that profoundly influenced the immigrant experience in Chicago Polonia. The first describes how Polish ethnicity became inextricably linked to the Roman Catholic faith. The second sadly recounts how internecine strife within Polonia and within Polonia's church permanently rent Chicago's Polish community during the mass migration years.

The general outlines of Chicago Polonia, Parot shows, were transplanted from Poland. A tradition of national parishes grew not only out of the exigencies of the polyglot Chicago diocese, but also from the close ties between Roman Catholicism and Polish nationality in the nineteenth-century Polish partitions. Similarly, a penchant for secular political organization and multivarious ideologies was another potent by-product of the Poles' stateless condition.

With this backdrop, the battles within Chicago Polonia raged. Some pitted Pole against Pole—as factional strife, personal antagonisms, regional rifts, and class tensions served to intensify the broader ideological struggle between clericalist and secular nationalist factions. In noting the last, Parot follows and fine tunes Victor Greene's interpretation in *For God and Country*. Other strife—over jurisdictional and doctrinal issues—aligned Polish Chicago against the Irish-dominated ecclesiastical hierarchy and its policies of Americanization and in favor of Polish representation in the hierarchy of the church.

The principal protagonists here were the Resurrectionist Fathers, a Polish missionary congregation that dominated Chicago Polonia during the mass migration years. Their history in Chicago followed a curious dialectic. The Resurrectionists overcame their secular nationalist opponents through a mutually advantageous accommodation struck with archdiocesan authorities. Ultimately, however, they themselves would face archdiocesan might and paradoxically also would counter by invoking Polish nationalism. In the process, they misplayed their

hand against Chicago's shrewd Archbishop Mundelein, but church authorities also erred by underestimating the depth of immigrant cultural and political nationalism.

For all that it teaches us, *Polish Catholics in Chicago* has its flaws nonetheless. For one, the book leaves a central question unanswered, namely, why were some lay Poles attracted to the clericalist party and others drawn to secular nationalism. Similarly, the author never establishes the demographic composition of either Chicago's Polish clergy or its Polish female religious, a fact one might presume rather crucial in determining their loyalties, attitudes, and positions during the period. Finally, the book's central theoretical concern stands largely unresolved. After the Vatican II reforms, "How is it possible to retain the ethnocentric character of Polish nationality while at the same time giving way to the centrifugal forces of Catholic universality" (p. xii)? Whither Polonia thus remains the nagging riddle.

As a social or intellectual history, the book therefore falls short. But as institutional history, Parot's densely written and thoroughly researched volume is a very respectable study whose oversights should find relief in a forthcoming sequel. Parot's next book, now in preparation, happily will chart the all-important connections between religion and society in immigrant Chicago, a task even more vital than the very useful work he has already accomplished here.

JOHN J. BUKOWCZYK
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MICHAEL D. CLARK. *Worldly Theologians: The Persistence of Religion in Nineteenth Century American Thought*. Washington: University Press of America. 1981. Pp. v, 321. Cloth \$21.75, paper \$12.25.

To *persist*, says Webster, is "to go on resolutely in spite of opposition, importunity, or warning." To use the word *persistence*, as an attribute of religion in nineteenth-century American thought, is thus to imply that the presence of religious thinking in that era is an anomaly that somehow has to be explained. The author of *Worldly Theologians*, Michael C. Clark, has been entrapped by an old positivist ploy, namely, that when one encounters religion in "the modern world" it is an archaic survival from its proper time in the sense of another Webster definition of *persist*: "to continue to exist or endure, as beyond a normal period or after the removal of a cause." But surely we do not have to argue that sterile chapter in American historiography all over again. If we have learned anything at all from Henry May's essay on "The Recovery of American Religious History" (*AHR*, 70 [1964]), and from the impressive body of

scholarship to which that essay referred, it ought to have been the fact that religion in nineteenth-century America was alive and well.

Some Americans of that era, to be sure, conceived of religion as something going on in the face of opposition rather than as a natural part of the landscape. In particular, American intellectuals of the post-Civil War "genteel tradition" felt that way. We have known for a long time, however, about the religious anxieties of the half-dozen thinkers Clark describes. They were introspective souls, who intensively studied not only the world but also themselves, and they were all highly enough placed in the cultural establishment to compel literary notice. And that is part of our problem with them. A graduate student in a Western state university once asked me in a moment of classroom irritation if I thought everything in nineteenth-century America had happened in Massachusetts! Such thinkers as George Bancroft, John Fiske, Josiah Royce, William James, and the Adams brothers may well represent the quintessence of Gilded Age Boston and Harvard, but they are not *synonymous* with "American thought" as the subtitle of this book inadvertently implies.

They are, nonetheless, weighty enough to warrant study; there is always something new to be said about William James. Far too often, unfortunately, when the author wants to express a judgment of a controvertible point he hides behind a cited critical authority. Although smoothly and clearly written for the most part, the narrative is irritatingly interrupted by long, clogging quotations. And some of the freshest lines in the book—little-known but pertinent statements by or about one or another of the six thinkers—are buried in the footnotes. In the same mail with this book came an advertising flyer from its publisher, pointing out that "the normal gestation period for a book . . . presents special problems for the academic community." I could have waited longer—quite a lot longer—for this one.

PAUL A. CARTER
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ROBERT S. FOGARTY. *The Righteous Remnant: The House of David*. Kent, Ohio: Kent State University Press. 1981. Pp. xiii, 195. \$17.50.

A photograph reprinted in this scholarly study of the House of David, a Christian millenarian sect that flourished in Benton Harbor, Michigan, between 1903 and 1927, aptly illustrates the group's disturbing mixture of the strange and the familiar. At first glance the photograph appears to be a nostalgic piece of Americana: three smiling young men on a small-town baseball field wearing the baggy uni-

forms of fifty years ago. But these players have immensely long, thick, tangled hair that covers their shoulders and extends down to their waists. And their bright smiles have that chilling, glazed quality we have learned to call "blissed out."

These men were indeed baseball players—the House of David sponsored a team of "long haired boys," which drew large crowds and earned substantial profits at exhibition games around the country—but they were also among the eight hundred devoted followers of one Benjamin Purnell. This self-proclaimed "Seventh Messenger of God" promised his followers that they would never die and that, after the imminent Second Coming, they would be transformed into "God Men," immortal rulers of heaven and earth. Meanwhile, they must give all their worldly goods to Purnell and move into his Benton Harbor "Shiloh"; practice poverty, chastity, and obedience to the "Messenger"; let their hair grow long; and play baseball.

We are indebted to Robert S. Fogarty for his serious and always scholarly approach to this unusual material. He has tried to place the House of David in the larger traditions of both religious millenarianism and American utopian communities. He has read widely in both fields; he has searched diligently for all relevant archival material; and he has made good use of both current writing on cults and utopias and of the best histories of millenarian beliefs. He has, for example, succeeded in tracing Purnell's prophecies—a near illiterate mass of confused ramblings—back through several obscure figures to Joanna Southcott, the English "Prophetess" whose chiliastic visions stirred up large sections of the English poor in the first two decades of the nineteenth century.

Fogarty has also given us a vivid and believable portrait of Benjamin Purnell, who surely deserves a place in the rogues' gallery of American prophet-charlatans. If his preaching was crude, he had refined the art of getting his hands on his followers' money and exploiting their free labor in a variety of profitable enterprises. In addition to the baseball team, the House of David owned farms, tailoring and printing shops, a cement factory, and an amusement park. Purnell and his wife "Queen Mary" enjoyed as much regal splendor as Benton Harbor could provide while the rest of the sect lived frugally on beans and potatoes.

Luxury was not all that Purnell craved. He revealed to a select group of young women followers that, while sex with ordinary mortals was sin and pollution, he was Jesus on earth whose touch purified. When Satan sowed the tares, the Son of Man sowed the good seed. Purnell's devotion to his sowing became so notorious that in November 1927 the state of Michigan secured a court order placing the House of David in receivership and expelling its

leader from the colony. A month later the prophet died.

Although Fogarty does not neglect the darker side of the House of David—indeed, most of his material comes from the judicial proceedings against Purnell—he seems uneasy with its implications. Recent historical writing on “communitarian” movements has emphasized their positive contributions, and Fogarty is constantly searching for evidence of “community” and “commitment.” What he finds is perhaps more disturbing than Purnell’s sexual adventures: children deliberately deprived of education; enforced “confessions” by the faithful that were secretly recorded and used to blackmail potential critics; a paranoid suspicion of outsiders, especially those former members known as “scorpions.” But Fogarty persists undaunted. “The public record,” he observes, “can often reveal what is wrong with such a society, but it cannot adequately convey what is right, how the group satisfied the social, emotional, and religious needs of the believers” (p. 80).

The House of David, like subsequent cults, presumably did satisfy such needs, but at what price? The rise and fall of Purnell’s millennium reveals an alarming pattern at the fringes of American life: the persistent appeal of the prophet-charlatan; the appalling credulity of his followers; the surprising strength of communities based on enforced ignorance, exploitation, and paranoia. On Fogarty’s own evidence, the House of David was closer to Jonestown than Jerusalem.

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ROBERT MAPES ANDERSON. *Vision of the Disinherited: The Making of American Pentecostalism*. New York: Oxford University Press. 1979. Pp. 334. \$16.95.

This book is a superb account of the origins of American pentecostalism. Robert Mapes Anderson notes the contributions of the nineteenth-century Keswick and holiness movements but correctly argues that “Pentecostalism is a movement that emerged on the world scene in 1906 during a Los Angeles revival in which speaking in tongues was regarded as a sign of Baptism in the Spirit. . .” (p. 4). The book ends in the mid-1930s after pentecostalism had fragmented into a bewildering assortment of sects. Anderson has done exhaustive research in the obscure pentecostal records of the period; his narrative is filled with challenging interpretations.

After a useful initial chapter surveying the historical, psychological, and theological literature on speaking in tongues, Anderson develops three themes. First, he tells the story of the growth and fragmentation of American pentecostalism in the

early twentieth century. The movement began in a 1906 revival at the Azusa Street mission in Los Angeles and spread rapidly across the country. Its abatement began two or three years later, and a period of “internal dissension and institutionalization” followed. Ever open to the “leadings of the Spirit,” pentecostals suffered countless schisms, including major doctrinal divisions over the relationship of sanctification and the Baptism of the Holy Spirit and the doctrine of the Trinity. Most of the small sects also divided along racial lines. By the 1930s, the pentecostal world seemed to be sheer chaos to outsiders, but Anderson carefully describes the debates and ties each schism to the diverse theological and class backgrounds of pentecostals. Anderson confirms that pentecostalism was not merely an offshoot of the American holiness movement; the movement also had a Calvinistic heritage. Neither was pentecostal culture uniform—pentecostals were Southern, Midwestern, urban, rural, black, and white.

Second, Anderson examines the class origins of pentecostalism. His conclusion is incontrovertible: “The pentecostal faithful were everywhere drawn from the humbler orders of society” (p. 114). Anderson does not argue simplistically that economic deprivation alone accounts for the rise of pentecostalism, but he does emphasize that pentecostalism grew in the context of poverty. He bolsters his interpretation with an interesting chapter based on the life stories of forty-five pentecostal leaders. The composite preacher that emerges was poor, ill educated, and often burdened with personal problems and a tragic past. All seemed to be trapped in the “limbo between working and middle class” (p. 108).

Finally, Anderson discusses the content of the pentecostal message. He notes that the early revival was primarily millenarian, viewing the Baptism of the Holy Spirit as a sign of the second coming. But once the early euphoria died away, the Baptism of the Holy Spirit, accompanied by speaking in tongues, became the centerpiece of pentecostal theology. More important, Anderson describes the “conservative and reactionary” social content of the pentecostal message. While early pentecostals sometimes strayed from social convention (as in their early interracial meetings), their intent was to reject the world, not to change it. While pentecostalism seemed to be a radical religious experience, for most of its poor adherents it was a “passive acquiescence to a world they hated and wished to escape” (p. 222).

Anderson’s conclusions are cogently argued. Whatever one may think of his emphasis on deprivation, however, this book must now become the standard account of the origins of American pentecostalism.

DAVID EDWIN HARRELL, JR.
University of Arkansas

ROBERT A. ROSENSTONE. *Romantic Revolutionary: A Biography of John Reed*. Reprint. New York: Vintage Books. 1981. Pp. xiv, 430, xiii. \$6.95.

WARREN BEATTY and TREVOR GRIFFITHS. *Reds*. Produced and directed by WARREN BEATTY. New York: Paramount. 1981.

"Reds" opens in 1915. John Reed was five years out of Harvard. He had already been the lover and traveling companion of Mabel Dodge, written a book of poetry, covered the slogging and senseless trench warfare overseas in desultory fashion. Of the last, nonetheless, Dois Passos observed in *Nineteen Nineteen*: "John Reed was the best American writer of his time, if anybody wanted to know about the war they could have read about it in the articles he wrote about the German front, the Serbian retreat, Soloniki . . . Reed was with the boys who were being blown to hell." He was 28.

The movie thus devotes itself to Reed's last half-dozen years. He was by then a legend in his lifetime, the Greenwich Village *wunderkind*. But the average moviegoer can only guess as much—or turn to Robert Rosenstone's illuminating biography, recently reprinted. He is advised to do so. For Rosenstone's is easily the best book on Reed and his age that we have—more thorough than Draper's brief study, more balanced and politically neutral than Granville Hicks's legendary profile of 1937, more perceptive than that of Richard O'Connor and Dale Walker thirty years later.

Offering both record and interpretation, Rosenstone's biography gives us what "Reds" neglected: the cosseted dreamer; the frail and sheltered son of patrician aristocrats of conservative, Bible-centered Portland; the child steeped in chivalric romances; the dull private school training of an outsider; the Eastern prep school mandated by the Northwest's social elite, a Morristown, New Jersey, academy at which Reed majored in sports and social activities; enrollment at Harvard where, again shunned—by the establishment of elite prep school graduates—he determinedly climbed the social ladder, in the process shedding a Jewish roommate (the future psychologist, Carl Binger) who might have impeded his ascent. He carefully avoided the newly formed Socialist Club and became almost preternaturally preoccupied with extracurricular affairs—beer-drinking, water polo, the *Lampoon*; and, having failed a football tryout, he emerged as charismatic team cheerleader. After graduation came wanderings through France and Spain, Greenwich Village in its fabled days, Mabel Dodge and the *coup de foudre*, publication of the carefree *Day in Bohemia*—which captured the joy and excitement of his first year in New York.

By now Reed was Max Eastman's golden boy, the spellbinder who spoke in the demotic tongue with

never a dead phrase, the privileged radical who, long before his sudden romance with proletarian revolution, sought the field of heroic action. Much like Theodore Roosevelt, an early idol, he longed for the strenuous, to taste life to the fullest, to overcome sickly youth, marginal athletic skills, uncertain masculinity, daunting obstacles—to be tested in some form: military combat, for Roosevelt; football, with its glamour and weekend heroism, for Reed. And much like another dashing hero of contemporary radicalism, Jack London, Reed spoiled for romantic adventure and held to robust enthusiasms. Indeed London quit the Socialist party because it was, as Reed described it, "duller than religion." Or, resembling Frank Norris, Reed worshipped his father, believing his own sickliness, cowardice, lack of athletic ability deeply disappointed B. J. Reed; and he, too, tried out for football driven, we may believe, by the same need to prove himself to the man who was an American success story. And joining with Mabel Dodge, so central to his early experiences in the Village, he also searched for personal authenticity and for what D. H. Lawrence, a future soulmate of Dodge's, called the "primal, dark veracity" underlying conventional culture. Reed would also plunge into the stream of experience, seek to live directly and spontaneously—to the outer limits of his potential. Dodge, to be sure, came to sentimentalize the primitive, and Reed did the worker—twin paradigms celebrated by the Village's lyrical radicals. The militant, the prole, the doer, the "deed," these took precedence over the ratiocinative. Suspicious of the authority of reason, culture radicals like Reed understandably gravitated toward the Industrial Workers of the World and, after October 1917, the Russian Revolution, each in turn becoming almost a definition of self as well as an ideological conviction.

Reed, then, as the film fails to convey, was symbol for an age, the embodiment of the restless romantic quest with its disdain for radical theory. He was the "Rover Boy," as Daniel Bell called him, who conveyed the historical romance of revolution in his poetry, essays, and reportage. Rosenstone understands him. Beginning with a fantasy that Reed spun out in 1908, one mingling loneliness and a dream of glory, he sketches a boy's feel of experiences. Though not psychobiography, *Romantic Revolutionary* explores illusions and intuitions—the chimeras of a secluded youth whose maturity came so slowly and painfully. Gradually, in the Village, art, love, poetry, radicalism, and the "enchanted" blended. Rosenstone then takes his subject to Mexico, which, except for one brief sequence that surely leaves most moviegoers bewildered, is neglected in "Reds." He describes—the film does not!—Reed's free-wheeling and umbilical attachment to Pancho Villa, depicted as a Robin Hood revolutionary, and to the saddle soldiers with whom Reed shared his

canteen. Missing, too, is the tour of the Balkan fighting front; the Ludlow strike, where besieged miners battled vigilantes on Rockefeller's fiefdom; the striking mill operators of Paterson's silk mills, whom Reed led in singing "The Internationale" and from whom he learned about the quotidian despair of working-class life; the commitment to the dual cause of birth control and the "new woman" who was then, as Eastman proclaimed, fighting for the "fullness of life." Nor does the film—how could it?—convey the rhythmic power of Reed's prose—whether the brilliantly alive *Insurgent Mexico* or the memorable *Ten Days*; or, for that matter, the aura that surrounded him—something finely incandescent that, radiating poetry and justice, shaped a myth even before his canonization in Moscow.

Warren Beatty, however, might have satisfied scholars and assisted his movie audiences by providing a few on-screen lines, in pretalkies fashion, before important sequences—possibly some succinct explanation of the Mexican Revolution, of Lenin's disfiguring polemicism, of the bitter Comintern quarrels over CP versus CLP recognition and over "boring from within" versus alliance with the IWW, of the fleeting and adventitious post-Baku military encounter; surely some identification of the "witnesses." The last is a bold device—reminiscent of Dos Passos's innovations in *U.S.A.*—that intersperses narration and connects dramatic events with the past, as glimpsed through the fog of memory. It helps us to situate Reed historically, but Beatty, perhaps unwisely, tells us nothing about these choric voices.

Rather Beatty places the love story front and center, especially in the film's first half, which emphasizes Louise Bryant's dilemma as a woman in love with a man who is often absorbed elsewhere. He also gives us any number of tedious clichés: the darling dog scratching at the door of the happily lovemaking couple, the slapstick kitchen mishaps, Reed hitting his head on the low-hanging chandelier in the Petrograd apartment; and some aesthetically cheap sequences, such as Reed chasing after the Red army caisson on the return trip from Baku, inserted to establish suspense since the audience, like Bryant herself at the platform vigil, now does not know whether Reed survived the military action. But, then, moviegoers are not given a sure sense of who Reed and Bryant were, of what the Village was, of who or what its residents aspired to. Taking liberties with Reed's life and the Reed-Bryant relationship, the film trades in some very marketable commodities—like contemporary feminism. Granted that it has at times a magnificent historical sense and that its focus on revolutionary and sexual politics is recognizably modern, "Reds" has some Reed-Bryant conversations sound as if hero and heroine were fugitives from a human encounter group. It perceives them, in Beatty's words, as

"liberals in a hurry," with all the "right" sympathies—the sort of retrospective symmetry that historians deplore. It redraws Zinoviev, in reality a pudgy and unpleasant man, but does manage a chilling portrait of the apparatchik. It includes some unexplained and seemingly unnecessary fictions—Reed chasing after caissons; going to France and, finding Bryant, persuading her that the "real story" is about to break in Russia; Bryant being fired by her news syndicate; her epic and interminable trek across frozen tundras (on her second Russian journey), going to the Finnish prison, searching for her beloved Jack; *omnia vincit amor*, Hollywood version, a commercially viable but hardly credible emanation of Bryant's character that in the film's first half was drawn as someone seeking "independence." She in fact left New York for Russia "bursting with guilt over an affair with a former lover of Mabel Dodge."

This all-American commercial sludge that intermittently drenches sequences of "Reds" cannot be waved away. And the film, it is clear, is less about a revolution and more about two people caught up in it. Indeed it is a careful film, avoiding the big radical questions and playing it relatively safe. But we still owe Warren Beatty a debt of gratitude. So much of the immense tableau is very impressive. No other major Hollywood motion picture in recent memory—with "Bound for Glory," the film about Woody Guthrie, a possible exception—has been so responsible to history, so celebrated American radicalism; none has rendered ideological commitments so credibly or revolution so sympathetically or the communist left so openly or a radical hero so winning. For all of the fashionable parlor criticism of "Reds," I can think of no other film that has conveyed an honest accounting of, say, the factional struggle between prowar and antiwar Socialists, pro- and antiunionists or, if superficially depicted, Socialists and Communists. When did Hollywood last convey the atmospheric political and journalistic shoptalk of culture radicals—even with the camera continually cutting back to Bryant's personal, non-political feelings? And when did it sketch vignettes of Villa's *los hombres*, the Winter Palace, Lenin at Smolny (like an animated socialist realism canvas), the Congress of the Soviets, the Bolsheviks who found power in the streets, the marching masses of Petrograd, the poetry and gnosticism of the Revolution. None, to my knowledge, has ever drawn the Socialist party's expulsion of antiwar militants, or the party split that carried from Chicago to Moscow, or the Comintern conclave and ukase, or the polyglot Baku gathering with its Islamic holy war overtones, or the Red versus White armies, or the witch-hunting investigations of the Senate's Overman committee in 1919.

These scenes flood out with generous and dramatic sweep, as "Reds" carries Reed/Beatty roughly from the time of his encounter with Big Bill

Haywood—which in Rosenstone's account, was crucial to Reed's discovery of a political identity—to Petrograd with Louise Bryant, to the ten days that shook Reed and the world, and to his later return—in order to argue for the CLP's cause as *the* officially recognized American Communist party; and then to his propaganda labors for world revolution, and to his death of typhus in a Moscow hospital at age 32.

Rosenstone does not concern himself with the question of Reed's opinion of the Revolution at the end of his life. Max Eastman, Emma Goldman, Ben Gitlow, Theodore Draper, *inter alios*, claim disillusion by 1920. Certainly, as "Reds" does show, Goldman did try to convince him of Bolshevik failings, of the growing political repression, and the activities of the CHEKA. But Reed, possibly because he knew that Goldman, being an anarchist, would find any bureaucratic state apparatus anathema, was unmoved by her doubts. He remained loyal, so it seems, to the dream of a known democratic heritage.

Nor does Rosenstone assess "what John Reed's life 'means'" or what it "says about America"; he leaves such matters to others. Dismayed that Reed was not "a more caring human being," and suggesting his personal and theoretical deficiencies, he chooses to emphasize his subject's "intelligence and social conscience"—which enabled Reed to transcend the anti-intellectual instinctualism that drove some contemporaries to adventurism *per se*. Reed never held a clearly formulated set of beliefs that might give substance to his revolutionary elan, but he embodied both the innocence and responsibility of American radicals. Rich in narrative, prodigiously researched, Rosenstone's book conveys this Reed with sensitivity, lucidity of judgment, and controlled affection for its subject. It is in the best tradition of American biography.

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EDWIN A. WEINSTEIN. *Woodrow Wilson: A Medical and Psychological Biography*. (Supplementary Volumes to the Papers of Woodrow Wilson.) Princeton: Princeton University Press. 1981. Pp. xi, 399. \$18.50.

This is a distinguished and stimulating piece of historical and medical research by Edwin A. Weinstein, a neurologist and psychiatrist, who has spent the last several years working on Woodrow Wilson. Despite the absence of medical records from Wilson's doctors, the extraordinary corpus of Wilson's papers makes this study possible and provides a significant amount of material about Wil-

son's physical and emotional health. Weinstein has drawn heavily from *The Papers of Woodrow Wilson*, edited by Arthur S. Link and others, and the result is a richly textured analysis of what Wilson himself recognized: "You know that if you be ill, nothing will go well with you."

Weinstein's overriding thesis is that Wilson's ideas and behavior were decisively influenced by his health at several key points in his life, especially during his presidency of Princeton University and his negotiations over the Versailles Treaty and the League of Nations. He argues that Wilson suffered from a form of cardiovascular disease and suffered several major and minor strokes, even prior to becoming president of the United States. But there is more—gastrointestinal and digestive problems, psychosomatic illness, phlebitis, a hernia, neuritis, headaches, and so forth. Weinstein maintains further that Wilson suffered from developmental dyslexia, which presumably accounts for the apparent delay in Wilson's learning to read and write. The chief strength of Weinstein's analysis is his treatment of Wilson's medical problems and suggestion of some of their psychological and behavioral consequences.

The book as a whole is weak, however, in several important areas. First, Weinstein's emphasis on physical and psychological factors sometimes blinds him to social, political, and economic forces that shaped Wilson's behavior and policies. The link between illness and behavior is at times too tightly drawn and leaves out the diverse influences that affected Wilson. Second, apart from his discussion of Wilson's health, Weinstein does not offer much new material on Wilson's life that was previously unavailable. He depends heavily on Wilson's published papers for most of the book, but since this series is still incomplete, his documentary evidence for the latter period of Wilson's presidency is not drawn from Wilson's private papers but from memoirs and secondary sources. Third, although Weinstein does offer some psychological interpretations of Wilson's use of language, they are tentative in character, and he could have pursued this avenue of inquiry much more boldly. This is particularly true of Wilson's expressions of his religious faith, perhaps the most symbolic area of his life and certainly one of the most important sources for understanding his personality and behavior.

Despite these weaknesses, Weinstein's book is an important contribution to the study of Woodrow Wilson and the modern presidency. After reading it, one is impressed anew with the achievement of this man but also with the all-too-human frailty that exercised power at a turning point in American and world history.

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JORDAN A. SCHWARZ. *The Speculator: Bernard M. Baruch in Washington, 1917–1965*. Chapel Hill: University of North Carolina Press. 1981. Pp. xvii, 679. \$27.50.

Ask an informed American above the age of forty to identify the similarities between Bernard Baruch, Walter Lippman, and Hyman Rickover, and he or she likely would reply that all led long lives, were active in public affairs while holding down nonelective offices, and had profound influences on public policy in several political eras. In addition, these men were deracinated Jews who suffered as a result (with Rickover having the most troubles on this score) and who demonstrated a relative lack of interest regarding such matters at times when more might have been expected of them. Finally, the informed American might know that all three possessed a high degree of intellectual arrogance, and that none was quite what he appeared to be when in his prime. Henry Kissinger would fit in well with this group, supposing his career develops as now appears the case. Some enterprising scholar, perhaps now in graduate school, might consider a collective biography of this quartet, to be undertaken around the year 2010.

Ask most literate Americans, of almost any age, what the first three have in common, and assuming he or she keeps an eye on the literary scene, the answer might be that all have been the subjects of massive biographies released over the past two years. Taken together, they would provide that historian of some three decades from now with a nice running head start.

Jordan A. Schwarz, whose *The Interregnum of Despair: Hoover, Congress, and the Depression* was well received a dozen years ago, seems to have spent most of the time since working on the present volume. The results are most gratifying. Schwarz has produced a work of impeccable research, carefully considered and supported judgments, and fine writing. In a different period it might have been subtitled: *The Man and His Times*. *The Speculator* is as much about why the United States developed a strong central government and turned from isolationism as it is a biography of its subject.

Baruch is presented here as a Wilsonian, part of that group (which included Franklin Roosevelt, Cordell Hull, Norman Davis, and even Robert Taft and Lippmann) that came under Wilson's aegis and remained true to the spirit for the rest of their lives, with each interpreting the faith in his own way. Baruch, of course, headed the War Industries Board, and ever after advocated economic planning and a strong central government. "He was not a democrat; he was a corporationist," writes Schwarz. Baruch favored programs set forth by an elite rather than by Congress. "The latter dilatorily

moved in concert with the interests of dominant pressure groups in their states or districts; the former, Baruch presumed, expeditiously acted in behalf of the national interest" (pp. 544–45).

One might have concluded from this that Baruch would have been more at home in the Republican than the Democratic party, but Schwarz carefully distinguishes between rhetoric and reality. In an illuminating section dealing with interwar politics, dissecting the Wilsonian legacy, Schwarz writes, "The rhetoric of voluntary cooperation usually sounded more natural coming from a Democrat than from a Republican. Baruch had chosen wisely in becoming a Democrat, whereas Hoover endeavored to apotheosize himself and his utopia above party" (p. 224).

Schwarz offers detailed presentations regarding Baruch's two great seasons in the limelight, World War I, when he was the nation's economic czar, and the early Cold War, when he helped shape the nation's role in foreign policy. But there also is a fine section on the Roosevelt-Baruch relationship. "Morally and ethically, Roosevelt believed that he was Baruch's superior; perhaps he was," writes the author (p. 417). I do not think Schwarz would have said as much had he known then of the recently revealed information that FDR had bugged Baruch's hotel room so as to catch him in dalliance with a young woman.

No historian who ever has worked with a famous person on an autobiography or biography should miss Schwarz's frightening account of Margaret Coit's experiences with Baruch. For that matter, this is essential reading for anyone seeking a deep knowledge not only of the man but also of the development of American political life in this century.

ROBERT SOBEL
Hofstra University

EMILY S. ROSENBERG. *Spreading the American Dream: American Economic and Cultural Expansion, 1890–1945*. (American Century Series.) New York: Hill and Wang. 1982. Pp. xi, 258. Cloth \$16.95, paper \$6.95.

Until now revisionist historians have largely confined their critique of traditional diplomatic history to an analysis of economic expansion and its ideological rationalizations. *Spreading the American Dream* marks a significant departure from this pattern because it attempts a broader synthesis of commerce and culture. This is a reflection, at one level, of an increasing amount of research into the sociocultural ingredients of American foreign relations; at another, it is indicative of a growing fascination among adherents of economic modes of interpretation with the noncommercial aspects of culture and society.

Emily S. Rosenberg contends that American expansion was dominated by an ideology of "liberal developmentalism" that used the rhetoric of peace, prosperity, and democracy to promote "Americanizing the world in the name of modernization" (p. 20). In elaborating this argument, she summarizes the standard literature on economic developments and, more originally, incorporates a wide assortment of social activities into her analysis. Newspapermen, philanthropists, and movie stars, among others, receive prominent display alongside capitalists in this catalogue of America's wide-ranging international involvements. These varied activities are integrated within a framework of changing public-private relationships, beginning with the tentative efforts of the fin-de-siècle "promotional state" and culminating in the powerful Cold War "regulatory state." In general, this work does an admirable job of synthesizing a complex and heterogeneous literature. In its treatment of corporate philanthropy, however, its depiction of the development of governmental information programs, and its use of the notion of "chosen instrument policy" to describe the government's reliance upon private bodies to achieve public purposes, developments are oversimplified to the point of caricature.

More serious are its interpretive problems. In proportion as this book extends the range of revisionist analysis it also compounds the conceptual difficulties of that viewpoint. Surprisingly, although Rosenberg pays a good deal of attention to cultural expansion, nowhere does she define culture or its significance. Culture is simply spliced with commerce and the two are treated in parallel rather than interdependently. Other revisionist authors have offered more subtle analyses than she on aspects of this question. Her definition of liberal developmentalism is also open to objection on the grounds that it is much too narrow.

But the greatest difficulty arises from her failure to relate socioeconomic and political processes. In making the argument that liberal developmentalism resulted inevitably in the formation of the Cold War national security state, the entire political dimension of experience, which is partly independent of the social, is completely ignored by Rosenberg. Expansion becomes a deceptively painless process in which disagreements over fundamental political questions are papered over as "tactical" disputes. Moreover, the problem of how liberal developmentalism applies to already developed nations, with their potent centers of authority, is never addressed. Consequently, for all its methodological innovations, this book suffers from a cramped and ultimately unrealistic sense of historical proportion that is characteristic of revisionist works as a genre.

Overall, however, it represents a welcome addi-

tion to the literature of American foreign relations and deserves a wide audience.

FRANK NINKOVICH
St. John's University
Jamaica, New York

ROBERT ALAN GOLDBERG. *Hooded Empire: The Ku Klux Klan in Colorado*. Urbana: University of Illinois Press. 1981. Pp. xv, 255. \$13.50.

Colorado provides an excellent opportunity for a microanalysis of the Ku Klux Klan. Although the Centennial State enrolled only about 1 percent of the national membership, events there mirrored those elsewhere. Spouting its curious mixture of patriotism, morality, and bigotry, the Klan rose quickly in the early 1920s and by 1924 was described by the *Denver Post* as "the largest, most cohesive and most efficiently organized political force in the State of Colorado today." Its leader was John Galen Locke, a fat, ascetic, egotistical, New York City-born physician whose adroit manipulations enabled the secret order to elect Klansmen as governor (Clarence Morley), mayor of Denver (Ben Stapleton), and United States senator (Rice W. Means). Nor were Colorado officials reluctant to demonstrate their allegiance to the Invisible Empire; the Denver police chief suggested that all Protestant patrolmen fill out Klan membership applications.

Hooded Empire focuses on four cities along the eastern edge of the Rocky Mountains and one on the western slope, where the Klan did not even begin organizing until 1924. Analyzing political campaigns, local leadership, and klavern activities, Robert Alan Goldberg has produced the first book-length study of the Colorado organization and the best state study of the Klan now available. His most important source was the official roster of Denver Klan No. 1, a document owned by the state historical society that was closed to researchers until 1975. Goldberg sampled almost one thousand individuals from the 16,727 names available to him and used city directories and other sources to measure occupation, age, mobility, military service, residence, and years in the community. The result is a rich mine of information.

Dividing Denver Klansmen (the information on other cities is less complete) into leaders, early joiners, and late joiners, Goldberg determines that the leadership and the early joiners were just a cut below the city's elite, while the much more numerous late joiners approximated the outside society, which in Denver meant that they were lower-level, white-collar workers and manual employees. Despite the macho rhetoric, only about one-fourth of Klansmen had served in World War I.

Goldberg interprets the Klan as a social movement rather than as an invidious collection of deviates, and he argues that its success was due to the interplay of four variables: local tensions, governmental responsiveness, quality of Klan leadership, and community perceptions. He finds that membership was a realistic response to legitimate grievances, and he points out that the Klan was occasionally a progressive force. Diversity was the key to its appeal because few people, Goldberg thinks, took out their ten dollars on the basis of a single feature.

Surprisingly, Goldberg concludes that "traditional explanations for the growth of the Ku Klux Klan do not fit the Colorado case." With a weaker resolve to upset existing theories, he might have concluded that his data are consistent with earlier studies. For example, he decides that the "zone of emergence" concept is inapplicable, although elsewhere he admits that Kleagles exploited real community tensions rather than events outside a man's immediate frame of reference and that "neighborhood conflict rather than distant dangers produced Klan growth." He dismisses the urban-rural dichotomy as "useless" even though his own figures indicate that Denver, with one-fourth of the state's population, accounted for more than half the Klan membership. These quibbles of interpretation aside, however, Goldberg has written a handsomely detailed and well-researched volume that stands as a model of scholarship on a difficult topic.

KENNETH T. JACKSON
Columbia University

DUANE VANDENBUSCHE and DUANE A. SMITH. *A Land Alone: Colorado's Western Slope*. Boulder, Colo.: Pruett. 1981. Pp. x, 337. \$16.95.

The western slope of Colorado is obviously distinctive. It enjoys (?) an extraordinary congeries of climates: its heights receive conspicuously more precipitation—and its lesser reaches considerably less—than do the eastward-facing portions of the state. The Continental Divide in Colorado is in fact a serious matter; it divides profoundly. Severe weather on one side suggests moderation on the other. Its high snows can be prodigious. As late as the 1920s it was rarely possible to cross the state in winter from east to west except by rail, while in summer the complete traverse by family touring car could require as much as three days. And the western slope's other boundaries, though artificially derived, serve also to set it apart: by coincidence they mark the endings, north and south, of serious mountains and, to the west, the beginnings of tablelands, largely deserts.

This is not a pioneer work; general studies seldom

are. It is a series of resums, depicting the activities, varying with time, of men and women in an awkward and beautiful land. The book is clearly historical; it proceeds from hazy beginnings through confident narratives to current uncertainties. But it is not especially scholarly, and, though it provides a more than adequate bibliography, the citations are sparse. Perhaps because there are two authors, its literary style is uneven.

But Duane Vandenbusche and Duane A. Smith do know their western slope, and it is difficult to discover substantial errors. That the Moffat Tunnel passes beneath James Peak has been asserted so often that one can hardly fault a reassertion. The expression "broad gauge" might better have been rendered as "standard," but this is common usage on the western slope. A curious deficiency is the omission of transdivide water diversions without tunnels, of which the most considerable is the Grand River Ditch from the North Fork of the Colorado to the upper Cache la Poudre.

A Land Alone is notably strong in its treatment of clashes of interest: aboriginal versus European, sheep versus cattle, cattle versus farm, slope versus slope. The slope contest has had notoriously to do with water, and chapter 17, which deals with this vital matter, deserves careful reading. Best of all, perhaps, is the evaluation of the latest issues, in particular the widely advertised struggle over the "environment." Here the authors are eminently fair, yet their sympathies are clearly with the conservationists. They cannot be blamed; anyone who has looked upon the western San Juan Mountains on a fine June morning will understand why.

ROBERT C. BLACK III
Tabernash, Colorado

NICHOLAS C. BURCKEL and JOHN A. NEUENSCHWANDER, editors. *Kenosha Retrospective: A Biographical Approach*. Kenosha, Wisc.: Kenosha County Bicentennial Commission; distributed by Archives and Area Research Center, University of Wisconsin, Parkside or Kenosha Public Museum. 1981. Pp. xvi, 384. \$10.00.

Bicentennial history was a thriving cottage industry in Wisconsin's southeastern corner. Starting with *Kenosha County in the Twentieth Century: A Topical History* (1976), edited by John A. Neuenschwander and followed by *Racine: Growth and Change in a Wisconsin County* (1977), edited by Nicholas C. Burckel, we now have the present volume edited jointly by Burckel and Neuenschwander.

The Racine book, a fat volume of 648 pages, proved that a topical approach, handled by ten authors, could yield readable, if somewhat disjoint-

ed and occasionally repetitious, history. *Kenosha Retrospective* illustrates the limitations of the biographical approach to local history. The editors found their initial idea, a representative selection reflective of the "new history," presented "insurmountable difficulties" (p. viii), which may be readily imagined. They shifted to a chronological selection of figures representative of industry, labor, and politics, based on their historical importance and available source material.

Zalmon P. Simmons, an authentic city father whose name became a national trademark—Simmons Beautyrest—is well handled by John Andreas, Jr., a Kenosha reporter. C. Fred Stemm, a labor leader of sorts, who had a long career in local politics around the turn of the century, defeated author Don Jensen. This was not necessarily Jensen's fault. Stemm simply does not offer many handles for a biographer; nor does "Felix Olkives: Labor Entrepreneur," for biographer Leon Applebaum. Running out of individuals, the editors selected a company biography and United Auto Workers Local 72; nine biographies in all.

Thomas J. Noer had the luck of the draw with Charles W. Nash, the creator of Nash Motors, which, as American Motors (AMC), has long been the precarious largest employer in Kenosha. Nash was an American original. A county ward who was "bound out" to an Illinois farmer at age six, Nash ran away at age twelve and hired out. Starting in 1890 as a cushion stuffer in a carriage factory, by 1910 he was president of General Motors. Nash decided that his style demanded that he own his operation and picked a small, failing Kenosha company, Jeffrey Motors, maker of Ramblers. Nash was a driver and skintight who thought he understood industrial workers because he imagined himself to be one of them. This is pretty much the story of Kenosha as an industrial suburb of Chicago and Milwaukee.

Another character of dimensions in this gallery is George Molinaro, son of an Italian immigrant, who went to work for Nash, in 1920, at age eighteen. Molinaro rose through the ranks of labor and ethnic-oriented local politics to a statewide reputation as the longtime assembly speaker, or alternately leader of the Democratic opposition. Elected to the assembly in 1946, he remained there for thirty-two years—sort of a local Sam Rayburn. Molinaro could have used some editing during his life, and also for the unleashed flood of bland quotes in this essay.

There is much in this volume that reaches beyond Kenosha, with its mix of ethnic backgrounds and large industrial payrolls controlled elsewhere. The book inclines one to the notion that collective writing of local history is a difficult art form. Pioneer Wisconsin cheesemakers solved a similar problem. The individual farmer could get his milk to clabber,

but it was difficult for an experienced cheesemaker to create a marketable product from the collective curds. That is why they started cheese factories, where the whole milk was delivered for the cheesemaker to clabber. They even sent him to the university to hone his skills. Kenoshans should know that.

ROBERT C. NESBIT
University of Wisconsin,
Madison

MORGAN SHERWOOD. *Big Game in Alaska: A History of Wildlife and People*. (Yale Western Americana Series, number 33.) New Haven: Yale University Press. 1981. Pp. xii, 200.

This stimulating and scholarly book, written by a specialist in the history of Alaskan science and exploration, Morgan Sherwood, is basically the story of the development of Alaskan wildlife management. In 1941 a legal controversy arose over hunting rights, when General Simon Bolivar Buckner, Jr., took the Alaska Game Commission to court, demanding the right to hunt on the same terms as any resident of Alaska. Buckner's action threatened the relationship of Alaskans with their natural environment; whereas most Alaskans hunted for food, Buckner wanted to hunt for recreation. By October 1941 nearly 23,000 soldiers, all of hunting age, were stationed in Alaska. Conservationists became alarmed, fearing the increase in hunters would threaten the game supply.

Buckner won his case since he had maintained his own residence off a military base. Although he claimed that his suit was a test case for the entire army, the court decision actually did not apply to servicemen stationed on bases. Once the United States became involved in the war, the game commission decided to give resident hunting privileges to servicemen upon completing a one-year tour of duty in the territory.

Prior to the Klondike-Alaska gold rushes there was little need for a comprehensive game law. A few trophy hunters then began to venture into Alaska. In 1901, the Boone and Crockett Club, a group of sportsmen dedicated to the preservation of big game, extended its campaign to Alaska where thousands of caribou, moose, and deer were being slaughtered. National sentiment in favor of preserving birds and wildlife in general and the political influence of the Boone and Crockett Club resulted in the passage of an Alaskan game law in 1902. The law prohibited the shipment from Alaska of any wild game with minor exceptions. A few United States marshals, customs collectors, and coast guard officers were to act as wardens. In 1908 a new law allowed the employment by the territorial governor of game wardens, who were empowered to register

game guides and to issue nonresident hunting and export licenses.

At first the Alaskan game wardens neglected their duties and appeared incompetent. Edward W. Nelson, chief of the Bureau of Biological Survey, sought reforms and was largely responsible for an improved Alaska Game Commission law enacted by Congress in 1925. The new law removed the appointment of wardens from the influence of local politics and provided for the appointment of commissioners representing the sectional opinion of Alaska.

Each of the ten chapters covers a specific subject such as the science of wildlife and technology of hunting, the native hunters, and the Euro-American hunters. In an afterword the author indicates that within Alaska both whites and natives wasted wildlife until 1925. By the 1930s the game commission's commitment to enforcement and to conservation limited the demand for game. Sherwood also suggests a plausible big game plan designed to preserve the animals in a healthy environment. The author's conservationist attitude toward the animals is evident throughout the book, but the reader will surely accept it. The book has a selection of thirteen apt illustrations, authoritative chapter notes, and a substantial bibliography.

BENJAMIN F. GILBERT
San Jose State University

THOMAS D. ISERN. *Custom Combining on the Great Plains: A History*. Norman: University of Oklahoma Press. 1981. Pp. xv, 248. \$14.95.

Americans have been a nomadic people. Whether pioneers, Forty-niners, sharecroppers, or migrant laborers, a restless energy has driven people to search for work and dreams. A largely ignored migratory group—Midwestern custom combiners—travel from Texas to Canada each summer, cutting wheat, oats, milo, and corn for local farmers. Thomas D. Isern, in *Custom Combining on the Great Plains*, provides a history of the origins and development of this unique business as well as a brief treatment of technological change in the wheat belt. Custom combining evolved naturally from custom threshing, which reached its peak during World War I. Large stationary threshers moved from farm to farm along with numerous laborers and bindlestiffs. With the perfection of the combined harvester in the 1920s, wheat farming modernized, as has most of American agriculture, by replacing hand labor with machines. Because combines were expensive, farmers increasingly hired experienced combiners. Custom combining assumed more importance during World War II. Launching a massive promotional campaign in 1944, the Massey-Harris Self-Pro-

pelled Harvest Brigade toured the wheat belt and cut over a million acres. Such supplemental combines proved necessary, for many wheat farmers lacked the capital to buy their own combines, and also the war created a shortage of new machinery. Strengthened by the successful war experience, custom combining became a way of life in the Midwest, continued through the hard years following World War II, and expanded in the 1960s.

Isern uses his sources well, tracing the movement of combiners throughout the Midwest, discovering their origins, describing their culture, and showing the economies of their trade. The typical cutter, for example, had a farm of his own, a stop in the journey from south to north, although others relied solely upon cutting for their livelihoods. Most crews were at the core family operations, though occasionally implements dealers furnished combines and hired crews. Depending upon combines for a living, custom combiners took pride in keeping their machines mechanically tuned and spotless. Isern discusses the daily and seasonal work culture of men and women on the crews. Women prepared the meals and washed the clothes while men serviced the machines and then drove them until wet wheat halted them, sometimes after dark. They often lived in self-contained trailer camps and had a largely unearned reputation for disorder.

To the historian, or anyone who has seen combines eating through Midwestern wheat fields or watched trucks transferring combines from one location to another and puzzled about the culture, Isern's book will prove informative. Documentary photographs, many taken from family albums, add a fresh dimension to the book. Having spent a summer in that work culture, I found his descriptions accurate, his approach sound, and his book a contribution to rural history.

PETE DANIEL
Woodrow Wilson International Center
for Scholars

EDWARD D. C. CAMPBELL, JR. *The Celluloid South: Hollywood and the Southern Myth*. Knoxville: University of Tennessee Press. 1981. Pp. xvii, 212. \$17.50.

The past decade or so has brought a number of books dealing wholly or in part with the depiction of American black people in motion pictures. Edwin D. C. Campbell, Jr., has a somewhat though not distinctively different focus: the way motion-picture makers have treated the American South in the years since Edwin S. Porter put together his remarkable version of *Uncle Tom's Cabin* in 1903. For Campbell, the elaborately contrived and sustained mythological South—either the South of benevolent whites and contented blacks portrayed up to the

1960s or of predatory whites and rebellious blacks in a number of later films—is actually less Southern than “distinctly American” (p. xiii). The celluloid South, in other words, has reflected the racial feelings and social yearnings of the majority of white Americans and, more recently in the “blaxploitation” films, the racial hostilities of urban blacks and the guilt of white liberals.

Campbell's focus turns out to be rather narrow, confined almost entirely to movie versions of the late slavery, Civil War, and Reconstruction South. But despite the dozens of movies up through the 1950s giving the “moonlight and magnolia” stereotype of the Old South and then the preoccupation with lust and violence in the “blaxploitation” epics, Campbell rightly insists that “No film has thus far adequately utilized both races as equally central to the theme of slavery” (p. 190). Moviemakers have generally been content to move from one ludicrously distorted version of the Southern past to another equally off the mark. More than any other medium, Campbell believes, film has established and perpetuated the image of the South as a place apart, essentially outside the mainstream of American life. “The survival of the South in the popular imagination owes more to the cinema than any other force” (p. 191).

Campbell does a fine job as far as his book goes. Not content to assess reactions to particular movies, he has done more than any other student of film I know to gauge moviemakers' intentions. His sources include a vast amount of press packages, lobby cards, posters, and other studio publicity materials. Yet Southern mythology, as formed and conveyed through motion pictures, involves considerably more than what Campbell has chosen to deal with. He does little more than mention significant alternative visions in movies like *Intruder in the Dust*, *The Defiant Ones*, *The Last American Hero*, and *Souther*. What about *Home from the Hill*, *The Long Hot Summer*, *The Well*, *Greased Lightning*—movies Campbell fails even to mention? Jack Temple Kirby's *Media-Made Dixie* (1978), with its thoughtful categorizations of Southern imagery and its coverage of fiction, television, stage drama, and popular historiography as well as films, offers a better understanding of how the South has looked to three generations of Americans in the twentieth century.

CHARLES C. ALEXANDER
Ohio University

LAURENCE M. HAUPTMAN. *The Iroquois and the New Deal*. (An Iroquois Book.) Syracuse, N.Y.: Syracuse University Press. 1981. Pp. xvi, 256. \$20.00.

This excellent monograph analyzes the impact of New Deal federal Indian policy on Iroquois tribes

living in New York, Oklahoma, and Wisconsin. It is the latest addition to the historical literature critical of Commissioner John Collier and the Indian New Deal. Laurence M. Hauptman explains how the Iroquois concept of sovereignty led to the overwhelming rejection, in New York, of the Indian Reorganization Act. Collier is portrayed as a romantic, vindictive, and condescending individual who failed to respect Iroquois treaty rights and conservative cultural traditions.

Hauptman offers a revisionist account of Alice Lee Jemison, a Seneca political activist, who used red-baiting and funds from extreme right-wing groups to discredit Collier's reform programs. Jemison emerges as a highly articulate critic. Influenced by the ideas of Carlos Montezuma, a Yavapai Indian, she wanted to abolish the Bureau of Indian Affairs. Jemison believed that the Indian Reorganization Act would strengthen the bureaucratic control of federal officials rather than provide the New York Iroquois with meaningful self-government. The author chastizes both Collier and Secretary of the Interior Harold Ickes for placing Jemison under FBI surveillance.

These criticisms are balanced by a discussion of how the Iroquois benefited from the Indian New Deal. Tribal reorganization helped restore a sense of political unity among the Wisconsin Oneida and Oklahoma Seneca-Cayugas where land allotment had severely weakened the idea of Iroquois sovereignty. These tribes used the Indian Reorganization and Oklahoma Welfare Acts to secure loans for the purchase of additional land and the financing of self-help endeavors.

Innovative New Deal relief programs left another positive legacy. The New York Iroquois obtained funds from the Works Progress Administration and National Youth Administration to build a community house at Tonawanda and develop a camp counselor training program for Indian youth. Arthur C. Parker, the director of the Rochester Municipal Museum, secured WPA appropriations to promote the Seneca Arts Project. Parker employed Indian artists to reproduce Iroquois arts and crafts for the museum's ethnological collection. Anthropologists from the University of Wisconsin used money from the Federal Writers Project to establish an Oneida Language and Folklore Project. Oneida Indians collected a record of their tribal history and worked with anthropologists to create an alphabet for use in native texts.

Hauptman provides only a brief glimpse of Iroquois history and culture prior to the 1930s. There is an abrupt transition between several chapters that have appeared elsewhere as published essays. The discussion of tribal reorganization among the Oneida and Seneca-Cayugas should have been placed in the context of the overall collapse of Indian reform

in Oklahoma and Wisconsin. The author does not fully explore the relationship between the successful campaign by Alice Lee Jemison and her associates in the American Indian Federation to discredit the Bureau of Indian Affairs and the postwar congressional policy of termination.

Well-researched, this book is based on a wide variety of archival sources and valuable oral interviews with Indians. Numerous photographs enhance the text. Hauptman has provided a significant contribution to understanding both Iroquois history and twentieth-century Indian affairs.

KENNETH PHILP
University of Texas,
Arlington

RALPH S. BRAX. *The First Student Movement: Student Activism in the United States during the 1930s*. (National University Publications Series in Political Science.) Port Washington, N.Y.: Kennikat Press. 1981. Pp. 121. \$17.50.

Student protest on American campuses had a significant effect on social and academic life during the late sixties, and the collegiate "movement" played a major role in the emergence of national antiwar activity. But Ralph S. Brax reminds us in *The First Student Movement* that three decades earlier student activism also was widespread. He argues that in the 1930s protest against American involvement in war was again the central feature of the movement and that "the overwhelming majority of the participating students had no relationship with any off-campus dissidents."

This first book-length study of the subject since the thirties offers interesting details on student upheavals at a variety of institutions. It reveals once more the curious mixture of passionate involvement in major issues and innocent collegiate "sand-box politics" that marked activities at different schools.

But this thin volume is marred by the limited scope of the author's research and the lack of space devoted to developing his thesis and to describing the growth of major student organizations. He is heavily dependent on primary sources from a handful of institutions in California and New York City. He does not fully assess the impact of off-campus radical politics, particularly the creation of the Popular Front in 1935 and its shaping effect on American communism. Brax notes how Communists came to dominate the American Student Union, and he describes the dramatic change in ASU policy from antiwar and antipreparedness activity to the endorsement of collective security against the spread of fascism. But many ASU student leaders were reflecting the change in the Communist International line. Were off-campus dissidents" really with-

out influence? The author's own account raises a central challenge to his thesis.

Indeed, this work does not delineate the ideological clashes among the members of many influential groups—the Young Communist League, the Young People's Socialist League, the factions within the American Youth Congress—fully enough to explain satisfactorily tensions within the larger movement from 1935 to 1939. Brax does better with the Student League for Industrial Democracy and the National Student League, but an analysis of the breakup of the antiwar Oxford Pledge coalition requires a closer look at key constituent groups than we are offered here.

While the antiwar orientation of the student activists is dramatically described in these pages, the author does not explain why the domestic crises of the depression decade did not inspire greater campus activism before and during the New Deal. And although he insists that the antiwar collegians "faced a military establishment with large resources of finances, influence and Congressional good will," the size, strength, and budgets of the army and the navy in the thirties suggest a very different conclusion.

A longer study might have addressed these questions, and with more space Brax might have described in greater detail how the tragic news from Spain and the shadow of Hitlerian imperialism created the final agony for *The First Student Movement*.

DAVID H. BENNETT
Syracuse University

WHITNEY T. PERKINS. *Constraint of Empire: The United States and Caribbean Interventions*. (Contributions in Comparative Colonial Studies, number 8.) Westport, Conn.: Greenwood Press. 1981. Pp. xv, 282. \$35.00.

The thesis of this turgidly written book, hinted at in the title, "Constraint of Empire," is that the United States, in undertaking an imperial role in the twentieth-century Caribbean, sought to create (or, in some instances, modify) political institutions in small countries to make future interventions unnecessary. Thus in the sometimes fervent historiographical debate over American involvement in the tropics, Whitney T. Perkins stands with Dana Gardner Munro and others who contend that American goals were essentially political as opposed to those who, like the older socialist critics Scott Nearing and Joseph Freeman or, later Gérard Pierre-Charles, portray the interventionists as the advance agents of American economic imperialism.

The author does not study every Caribbean country that has come under American tutelage. Instead

he focuses on four—Cuba, the Dominican Republic, Haiti, and Nicaragua—that have often felt the sometimes heavy hand of American military and political pressure and, in the first third of the twentieth century, experienced American occupation. Panama and Puerto Rico, in Perkins's analysis, are special cases that do not readily fit what he sees as the pattern of American empire. Yet, if not offering a *tour d'horizon* of the entire Caribbean, Perkins does provide a sweeping chronological perspective from the 1906 Cuban occupation, which civilian policy makers in Washington insisted on calling a "pacification," through the 1965 Dominican intervention.

Despite its limited goals, American efforts in the Caribbean never achieved the original intention of implanting strong political institutions in these countries, and in virtually every instance the U.S. wound up relying on existing political systems, as in Nicaragua, to achieve its purposes. Even much later, after the American government had professed its commitment to nonintervention, so strong was the older belief that its security interests in the Caribbean justified taking action if political disorder reached a dangerous level, that the U.S. did not hesitate long to revert to its interventionist ways, as it showed in the Dominican Republic in 1965. Yet even with twenty thousand troops in Santo Domingo, the American government realized, as it had recognized in the earlier interventions in Cuba and Nicaragua, the "constraints" upon its power. What Perkins offers, then, is a non-Marxist theory of empire explained by recapitulating American political goals and frustrations in these four countries. He is, admittedly, heavily obligated to the work of other scholars, notably Munro, and almost embarrassingly reliant on the *Foreign Relations* series of the Department of State, short quotations from which disfigure the narrative. The result is a book that, although by no means a State Department manifesto, reflects the view of empire shaped in Washington. The "real" American empire in the Caribbean—the empire carved out by banana barons and entrepreneurs and policed by American gunboats, and, after 1933, when the American government professed noninterventionist beliefs, maintained by the unwritten alliance between the U.S. and its Caribbean surrogates—perished in the ashes of the Nicaraguan civil war of 1979. That empire is scarcely discernible in this book. Caribbean peoples judge American rule not so much by what we wanted to do in their lands but by what we did.

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RICHARD A. LAUDERBAUGH. *American Steel Makers and the Coming of the Second World War*. (Studies in

American History and Culture, number 20.) Ann Arbor: UMI Research Press. 1980. Pp. 266. \$34.95.

Richard A. Lauderbaugh says he started out with a deceptively simple question about the relationship between businessmen and war. He winds up finding out things he never expected. Both from the book's appearance and length the reader expects to find, on the other hand, a quiet little monograph. If Lauderbaugh was surprised, so will his readers be. This is a first-class study based on wide reading in industry journals, academic periodicals, government documents, and key manuscript collections.

The book begins by setting forth several theories about the nature of the relationship between business and the government in capitalist society. How many dissertations begin in just this fashion? But then things start to happen. Very soon after the reader sits down to listen to an exposition of these theories and their inevitable flaws, the information on the pages begins to fall into a dramatic pattern. The central and overriding question for steel makers was the problem of how to maintain price stability. Everything came second to that. Yes, the steel makers were interested in expanding exports once the war began—but not if that meant expanding plant capacity.

Fearing that FDR and his latter-day economic theorists, Thurman Arnold, Leon Henderson, Isador Lubin, among others, were out to "TVA-ize" the industry (if not to nationalize outright), the companies were determined to resist the creation of excess steel capacity and to demonstrate their ability to meet the nation's military needs.

When Roosevelt called upon Edward R. Stettinius to help him break down the "bottleneck" that appeared to be developing in 1940, the former U.S. Steel executive turned over the problem of investigating industry capacity to a very friendly sort of fellow, Mr. Gano Dunn, whose report said that everything was just fine. This caused a great stir within the inner circle of Roosevelt advisers, but the president seemed inclined to take the report in good faith. In any event, he did not challenge the steel makers directly.

Why, asks Lauderbaugh in the second part of this book, were the industry moguls so conservative? Was it that they feared the New Deal mood would prevail even after the war? Partly that, it appears, but there was something even more fundamental. By the end of World War I, American steel makers were in a confident mood themselves and inclined to show their new muscles to those other puny fellows on the beach. Very soon, however, this confidence gave way to decided doubts about the future of the industry. Instead of assumptions about taking over the world market, American producers began to have fears about their ability even to maintain dominance of the domestic market.

Europe never recovered after World War I, but its export push in steel caused Americans real problems. These were exacerbated by the depression, the British Imperial Preference system, and the reorganization of the German industry, both before and after Hitler came to power. Faced with growing demands from the CIO's organizing committee for higher wages, U.S. Steel finally decided that it had but one option: to grant the demands and join the newly refashioned international steel cartel.

The essence of the decision was to turn American steel makers away from thinking about exports (an increasingly less important concern in these years anyway) to protecting the home market. As everyone knows, such "private" efforts to resolve international problems failed, and the war came. What turned the steel makers toward a strong foreign policy, then? The desire to expand foreign trade? Not at all. It was the power of German mobilization in the economic realm and fears that once the war ended American steel would face a dramatic challenge from noncapitalist sources. Even during the Battle of Britain, for example, the American steel men were impressed by Germany's ability to mount an export offensive.

Lauderbaugh makes very modest claims for this book, asserting only that like most efforts to test a hypothesis it disproves more than it proves. That may be so, but he has produced a book that will set others to thinking and rethinking a whole series of assumptions. A review of this length can only touch on a few of these. Reading this book may very well turn out to be a very profitable experience for just about anyone.

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JAMES LEUTZE. *A Different Kind of Victory: A Biography of Admiral Thomas C. Hart*. Annapolis, Md.: Naval Institute Press. 1981. Pp. xi, 362. \$21.95.

Admiral Thomas C. Hart was sixty-two when he took command of the Asiatic Fleet in the summer of 1939. A strict, dry, self-contained old veteran, he had served in the Spanish-American War and World War I, had commanded submarines and been superintendent of the Naval Academy, and was a gifted sailor and able administrator. When he shifted his flag from Shanghai to Manila in late 1940, he commanded a small force of old cruisers, destroyers, and submarines clearly inadequate to stand up to overwhelming Japanese naval power. Stripped of protective air cover in the opening days of the war, Hart fell back before the Japanese onslaught to the relative haven of the Netherlands Indies. There he assumed naval responsibility for

the newly formed Allied command but was soon forced to step down under controversial circumstances still subject to debate. He sat out the war in Washington, retiring in 1945 to accept appointment as a U.S. senator from Connecticut. He did not seek election when his brief term expired but settled into a long retirement until his death in 1971. James Leutze's solid, well-written biography of Hart is a welcome and overdue study. Based primarily on Hart's diary and other personal and official material, it fills an obvious gap in naval bibliography, especially on the early days of World War II in the Pacific. Hart has been the subject of considerable criticism both from General MacArthur, who accused him of mishandling the Asiatic Fleet at the start of the war, and from the Dutch and British, who have questioned his role as Allied naval commander in early 1942. The admiral has had his defenders, but we have not previously been afforded such a full presentation of his own side of the story.

It is, by and large, an effective defense, despite Leutze's clear sympathy for Hart and his unconvincing speculation (and Hart's belief) that President Roosevelt set the admiral up to provoke a Japanese attack. It is also unfortunate that Hart's irascibility and stubborn political conservatism do not make him the most lovable of heroes. But Hart, as this account makes clear, was a steady helmsman in a very rough sea. The force he commanded was too weak for its mission, he lacked precise instructions from Washington, he had a terrible problem in dealing with MacArthur, and his relations with the British and Dutch were frustrating and awkward, to say the least. Nevertheless, he did a fine job of organizing and commanding his limited resources, saving what he could in an impossible situation, and maintaining his dignity and sense of honor where others might easily have failed. Leutze's biography is worthy of its subject.

STANLEY L. FALK
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DAVID REYNOLDS. *The Creation of the Anglo-American Alliance, 1937-41: A Study in Competitive Cooperation*. Chapel Hill: University of North Carolina Press. 1981. Pp. xiii, 397. \$28.00.

Anglo-American relations in the late thirties and early forties is not a neglected subject. Memoirs, biographies, official histories, general diplomatic studies, and monographs on a host of subjects—appeasement, isolationism, interventionism, trade, finance, naval relations, intelligence gathering, the destroyer-bases deal, Lend-Lease, the Atlantic Conference, and the coming of the Pacific war, to

mention just some—have all moved across this terrain.

Undaunted by this intimidating body of work, David Reynolds, a fellow of Gonville and Caius College, Cambridge, has retold the story of British-American relations, 1937–41, in a book that should now be considered the premier work on the subject. Reynolds has no new, sensational revelations to offer on secret British discussions about peace overtures in 1940 or American conspiracies to enter the war. His study is not punctuated by startling revelations that dramatically change our understanding of what happened in these years. Rather, he has written a lucid, deeply researched volume, which subtly traces the many twists and turns in relations. Building on the vast body of printed studies and archival sources in Britain and the United States, Reynolds comes closer to recreating the ups and downs, the changes and continuities, the very smell and feel of Anglo-American exchanges during these five years than anyone before him. This is history “as it actually happened.”

The underlying theme of Reynolds's book is “competitive co-operation,” “the evolution of a unique alliance and, within that, the continual manoeuvring for advantage which was part of the shift of world power from Britain to the U.S.A.” (p. 3). Sentimentalists, he correctly points out, have seen relations in these years as altogether too friendly, while recent scholars have given too much weight to differences. Reynolds strikes a successful balance between these two overdrawn views, effectively demonstrating that both impulses operated at the same time, rising and falling in intensity as events changed.

In the late thirties, for example, British policy toward America was an amalgam of doubts, hopes, and fears. British leaders were simultaneously cynical about immediate significant help, hopeful of long-term assistance, especially in a war, and fearful that such help would come at the expense of British economic and political interests. On the American side, one finds not clear-cut trends but incoherence and ambiguity, the expressions of a nation and a government antagonistic to Nazism and determined to avoid involvement in another European war. The onset of the war in September 1939 initially encouraged all these feelings. Though the British looked forward to full American support if the war turned against them, they resisted “over-dependence” on America in the first six months of the fighting as likely to injure their long-term economic interests. Similarly, though the Roosevelt administration wished to supply the arms that could destroy Hitler, it feared commitments that would compel military steps to save the Allies.

The collapse of France in June 1940, however, altered this balance. Though doubts remained and

continued to trouble British dealings with the United States throughout the war, the need for American participation in the fighting to assure Britain's survival forced fears of excessive dependence on the U.S. into the background. Likewise, on the American side, though enduring suspicions of Britain continued to inform American thinking throughout the war, the Roosevelt administration pushed antagonism to British imperialism aside and committed itself to a share in the fighting. Yet American entrance into the war, Reynolds concludes, was not the result of any conspiracy but of a genuine surprise attack at Pearl Harbor. Anyone who finds the renewed accusations about a Pearl Harbor conspiracy persuasive would do well to read Reynolds's account of the events leading to the Pacific war. It is an excellent statement of the idea that fighting in Asia came not through Anglo-American design but as a result of Japanese ambitions and miscalculations on the American side.

There are also a number of points on which one can take issue with Reynolds. His conclusions about FDR's general approach to foreign affairs, particularly his Wilsonianism, the causes of his failure to revise the neutrality law in 1939, the meaning of the Lend-Lease act, the point at which Roosevelt decided that the United States would have to participate in the fighting, and American motives for the Atlantic Conference of 1941 are all very much open to debate. But anyone familiar with the diplomacy of the Roosevelt period would not find this surprising. The uncertainty of the times and the ambivalence with which Roosevelt responded to events probably make it inevitable that historians will continue to disagree about major historical episodes in this era. Indeed, this is much the point of Reynolds's book. Anglo-American relations between 1937 and 1941 is more a story of human confusion and contradiction than of orderly change in world affairs.

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ROBERT M. HATHAWAY. *Ambiguous Partnership: Britain and America, 1944–1947*. (Contemporary American History Series.) New York: Columbia University Press. 1981. Pp. x, 410. \$25.00.

This readable book seeks to shift the historian's perspective on the climax and aftermath of the Second World War away from the prism of Soviet-American confrontation toward that of the significance of Anglo-American relations and to show how “British desires became translated into United States foreign policy” (p. 2). While not denying Anglo-American discord or the enormous advantage in

national power and wealth that gave Americans the dominant hand in negotiations, Robert M. Hathaway shows that the Americans sometimes sought to aid rather than beggar their ally and that cultural ties facilitated subtle diplomatic understandings. Hathaway also evenhandedly criticizes diplomatic visions and policies on both sides of the Atlantic. Yet the author's own evidence suggests that throughout 1944-47 there persisted that fierce Anglo-American rivalry for political, economic, and military advantage that had become so prominent after the First World War; that Presidents Roosevelt and Truman pursued similar anti-imperial policies ("Coercive Liberty") toward the British, who in their Victorian conceit consistently overestimated American largesse and the propriety or need for American policy to suit British interests; and that American and British diplomats pursued parallel (not joint) policies chiefly to oppose the Soviet Union or left-wing revolt against right-wing authority.

Hathaway notes that in November 1944 the British sought \$7 billion in first-year Phase II Lend-Lease, while Roosevelt generously agreed to \$5.5 billion but refused a formal agreement to keep the British dependent on American good will. Similarly, Roosevelt never drew an agreement to implement his pledge for postwar military and civilian atomic energy collaboration and infuriated the British by refusing to develop joint policies before the Yalta Conference, where he shocked Churchill by according Stalin equal status, shocked Stalin (a good imperialist) by proposing that the British return Hong Kong to China, and then barred the British from deliberations about Far Eastern concessions for the Soviets. More significantly, Hathaway writes, the Americans initially aimed the Declaration on Liberated Europe equally at the British and Russians, while American reparations and dismemberment policies for Germany prevailed over British views. Afterward Roosevelt expressed as much concern about the British as about the Russians.

President Truman followed similar liberal but anti-imperial policies. Hathaway judges that Truman's 1945 Lend-Lease cutbacks reflected political demands for financial restraint that even FDR would have had to accept and that the terms for the \$3.75 billion loan to Britain were generous, even if the Americans cracked the sterling bloc by insisting on currency convertibility, nondiscrimination against American goods, and the Bretton Woods agreements. Truman, like Roosevelt, declined Churchill's request to assail the Soviets' "Black Curtain" in Europe and refused a pre-Potsdam meeting to avoid ganging up on the Soviets, while even Undersecretary of State Joseph Grew labeled the British agenda a bill of complaints against the Russians. At Potsdam Churchill cheered American disagreement with the Russians regarding the Polish

boundary, German reparations, and the Balkan governments, but the Americans determined their policies independently of the British who, especially after Clement Attlee replaced Churchill, saw the plenary sessions as meetings of the "Big Two and a Half."

Truman preferred not to share atomic information, and Hathaway notes that despite the November 1946 Anglo-American-Canadian agreement for full cooperation, no implementing agreement followed. A later memorandum of intention provided only that information about atomic plants would be negotiated on an ad hoc basis, while Congress ended foreign collaboration with the McMahon Act in 1946.

But nothing embittered Anglo-American relations more than the turmoil over whether to admit Holocaust survivors to Palestine or to create a Jewish homeland. Hathaway suggests that the passions and complexities of the problems might have defied peaceful solution, but he criticizes both the British for their unconcern about Jewish refugees and their "obsession" about the dangers of alienating the Arab world and the Americans for neither lowering their immigration barriers nor providing financial or military peacekeeping support. However, Hathaway unfairly charges Truman with political irresponsibility for his October 4, 1946 (Yom Kippur), statement, alleging that the president called for the "immediate immigration of 100,000 Jews" and virtually supported "the Zionist demand for a separate state in Palestine, a position no administration in Washington had ever before taken" (p. 285). Actually, Truman called for immediate "substantial" immigration and said he could not believe that "men of good reason" could not bridge the gap between the Jewish Agency call for a "viable" Jewish state and other provincial autonomy or binational state proposals then under consideration. Further, Truman's statement did not exceed either the 1944 Republican platform call for unrestricted Jewish immigration and landownership in Palestine to fulfill the intent of the Balfour Declaration nor the Democratic call for unrestricted Jewish immigration and colonization and establishment in Palestine "of a free and democratic Jewish commonwealth," a commitment that Roosevelt reiterated in October 1944 and March 1945. As a senator and early in his presidency, Truman espoused similar views; his 1946 statement, coming after adjournment of the Palestine Conference in London, might better be seen as reflecting his intense moral concern about the "terrible ordeal" of Jewish refugees and the failure of "men of good reason" to make progress on the intertwined refugee and homeland problems.

The book closes with the events of the terrible winter of 1946-47 and the American takeover of

"Britain's imperial responsibilities" in Greece and Turkey (p. 306), which Hathaway cites as evidence of the convergence of American and British policies but which might also be seen as the result of American expansion into the eastern Mediterranean and containment of the Soviet Union. Whatever the historian's judgment, Hathaway's valuable study provides significant insight into the ambivalence that characterized relations between these two ostensible allies and often seemed to suggest that diplomacy is war by other means.

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AARON DAVID MILLER. *Search for Security: Saudi Arabian Oil and American Foreign Policy, 1939-1949*. Chapel Hill: University of North Carolina Press. 1980. Pp. xviii, 320. \$19.00.

This book analyzes the bureaucratic, economic, and national security influences on the development of American policy toward Saudi Arabia between 1939 and 1949, a period, according to Aaron David Miller, when war and Cold War moved Saudi Arabia to the center of American security perceptions and when policymakers in Washington laid the foundation for a special relationship between the Saudi kingdom and the United States. Miller delineates three overlapping stages of policy development. During the first stage, roughly through 1942, considerations involving the strategic location and rich oil deposits of the Arabian peninsula led American officials to reinforce Great Britain's historic hegemony in that area. By 1943, however, these officials had concluded that Saudi oil was too important to be left in British hands, or, for that matter, in the hands of the oil companies and the Saudis. Anxious to conserve oil reserves in the Western Hemisphere and suspicious of the British, they now sought to reorganize regional relationships in a way that would make the United States, rather than Britain, the senior partner in an Anglo-American condominium over Saudi Arabia. Many of their plans, particularly those for American government control over Saudi oil and for long-term financial assistance to the Saudi government, faltered because of bureaucratic disputes and opposition from both Congress and the oil companies. But in 1944 the State Department managed to negotiate the Anglo-American Petroleum Agreement that protected American oil concessions in Saudi Arabia, upheld the open door principle, and established guidelines for the orderly development of international petroleum reserves.

As Miller sees it, American policy entered a third stage after the war. During this period old worries over the depletion of domestic reserves merged with

Cold War considerations and European oil requirements to create a new sense of urgency in those policymakers who wanted to bolster American prestige and power in Saudi Arabia. This explains their opposition to Truman's policy on Palestine, a policy, they worried, that might jeopardize American oil concessions in Saudi Arabia, alienate the Arab states, and open the door to Soviet penetration in the Middle East. It also explains their willingness to tolerate the restrictive postwar arrangements negotiated by the major oil companies. These arrangements violated the open door principle, but they also enhanced the American position in the Middle East and offset the ill effects of Truman's Palestine policy. In their response to the Palestine problem and the open door issue, Miller finds different reflections of the concern for national security that unified diplomacy toward Saudi Arabia after 1939.

Miller's book is a fine example of traditional diplomatic history. One could hope for a more sophisticated analysis of the relationship between the oil companies and the government, an analysis that goes beyond a conventional description of latter-day dollar diplomacy. Such an analysis, it seems to me, is essential if we are to understand the nature of American economic foreign policy in the age of multinational enterprise. Nevertheless, this is an excellent monograph; when combined with the new work by Michael Stoff, Burton Kaufman, and Irvine Anderson, it gives us our first serious examination of a very important facet of recent diplomacy.

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GAIL E. MEYER. *Egypt and the United States: The Formative Years*. Rutherford, N.J.: Fairleigh Dickinson University Press. 1980. Pp. 230. \$18.00.

Gail E. Meyer has creditably analyzed a significant segment of American foreign relations and recent Middle Eastern international politics in this monograph, which grew out of a University of Geneva doctoral dissertation, "American-Egyptian Relations, 1952-1958." The author has worked as a journalist in the Middle East and as an economist with the United Nations at Geneva. She orients the reader with pertinent highlights of the Egyptian past and Egyptian relations with the United States down to the young officers' revolution of 1952. Seven chapters follow "to disentangle the many strands that are interwoven in the complex history of the region during this period" (p. 10): the role of the United States in the resolution of the Anglo-Egyptian disputes; the way the Arab-Israeli tensions complicated Egyptian-American relations; the strategic goals guiding the United States in its desire to

arrange a Middle Eastern defense organization; Egypt's successful use of neutralism to protect its interests; the rise and decline of American commitment to share in financing the Aswan high dam; the American tilt toward Egypt during the Suez crisis of 1956; and the attempt through the Eisenhower Doctrine of 1957 to blunt Soviet influence in the Middle East. Although the thematic organization makes for considerable repetition, since it forces the author to move backward and forward in time while examining each facet, there is a gain in the resulting demonstration of the interrelatedness of the issues.

For the first two-and-one-half years following the 1952 revolution, there was optimism about a friendly relationship with the new Egyptian regime. Meyer assigns high marks to incoming Secretary of State John Foster Dulles for his initial understanding of Middle Eastern issues following his fact-finding mission of May 1953. After analyzing the promising beginnings, Meyer devotes the bulk of the book to the decline "on a zigzag course that often played havoc with America's global cold war strategy . . ." (p. 45).

Like previous writers, Meyer sees the Israeli raid into Gaza on February 22, 1955, as a turning point, followed by "a chain reaction of increasingly violent confrontations" (p. 78) that culminated in the Suez war. She deplores the American leadership's excessive fear of adverse public reaction at a time when, according to her controversial speculation, pressure on the Israelis might have opened the way to a compromise peace.

The chapters on Egyptian neutralism and the tangled beginnings of the Baghdad Pact are especially valuable summaries. In both, as in the discussion of the Aswan high dam project and the Suez war, the author surmounts the unavailability of important classified official sources by enterprising and shrewd use of the Dulles oral history collection at Princeton, newspapers, memoirs, and interviews with diplomats and journalists. This ingenuity is particularly helpful in illuminating Egyptian perspectives and actions.

The book's subtitle may mislead those who interpret "the formative years" as promising a study of the century or more prior to the 1950s. And one can wonder why, in the final chapter on the Eisenhower Doctrine, the author avoided dealing with the Iraqi revolution and the American intervention in Lebanon. Otherwise, with few exceptions, Meyer's judgments and inferences are well grounded.

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RUSSELL D. BUHITE. *Soviet-American Relations in Asia, 1945-1954*. Norman: University of Oklahoma Press. 1981. Pp. xiii, 254. \$14.95.

Russell D. Buhite is the author of several important works in the area of American-Asian relations, and this is another worthy contribution. Half a dozen chapters retell the tale of the principal elements of the Cold War in East Asia; early and futile efforts to use the USSR to help preserve a noncommunist China; the "specter of Soviet domination of China" and, therefore, aid to the Kuomintang as well as a later reappraisal of Taiwan and the decision to "save" that island; "excluding the Soviets" from a share in occupying and remodeling Japan; various futile attempts to create a unified, democratic Korea, culminating in humiliating defeat north of the thirty-eighth parallel; the evolution of Vietnam from a remote and peripheral interest to either a "major" interest or a "quasi-vital" one.

Drawing on private papers, oral histories, and most useful secondary works, Buhite makes each of these tales a work of art, getting to the core of each in clear, well-wrought prose. In each he reveals not only the American decision-making process but also the episode's impact on American minds, as in Korea's militarizing our foreign policy and providing us "lessons" for Vietnam.

The most significant contribution of this work, however, is Buhite's cogent warning regarding national interest. He reminds us of the two traditional varieties, the "vital" interests that relate directly to American survival or the "peripheral" interests that are not likely to affect either our immediate or long-range well-being, and he adds a new category. "Major" interests as Buhite sees them emerging in the Cold War are those that *might* (my emphasis) affect national well-being to a considerable extent and *might* require the use of substantial resources to defend. But, he points out, such in-between interests are difficult to recognize, transitory, changeable, and hence treacherous. Vietnam is obviously the perfect example.

Buhite also points to the problem of success in one area being used as a wrong model. American response to the Berlin blockade of 1948-49, he argues persuasively, was successful enough to cause exhilaration, a sense of triumph, and the perceived need to stand firm everywhere. Policy makers, he reveals, began to rethink once-peripheral interests and make them "major" interests; officials would "apply firmness far too unselectively" (p. 232). The crisis also reminded Americans of an old paradigm, the Munich Conference, and caused the overplaying of the notions of prestige and credibility. Taiwan, Korea, and Vietnam became places to stand firm rather than remote spots unlikely to affect American survival.

Buhite rightly avers that American policy makers were neither mental deficient nor paranoids, and they had a reasonably good understanding of Stalinist aims. Under the influence of the Berlin success

and their shifting of levels of interests, however, they led us astray. The chief lesson of this book? It is "that major interests [not *vital* ones] must not be militarized and that imponderables like credibility and prestige are not adequate grounds for military intervention" (p. 234).

As one would expect from this author and press the bibliography and index are useful, but the one map is too small, poorly reproduced, and of little value.

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JOHN LEWIS GADDIS. *Strategies of Containment: A Critical Appraisal of Postwar American National Security Policy*. New York: Oxford University Press. 1982. Pp. xi, 432. \$25.00.

With the proliferation of available records the problem of synthesizing the main currents of United States foreign policy in the era of the Cold War has become increasingly difficult. This study of the successive containment strategies of the United States after 1948 has resolved the problem of choice between the specific and the general by addressing a series of questions fundamental to any study of national strategy: How did each administration define the country's interests abroad? How did it perceive the threats to those interests? What responses did it select to defend those interests? How did it justify its responses? Through such questions John Lewis Gaddis has distinguished and analyzed five successive geopolitical codes that together defined the nation's response to the Russian and Communist challenges of the postwar period. Gaddis opens his progression with an acute and detailed analysis of George F. Kennan's original concept of containment. He then proceeds through a series of modifications: the Truman administration's NSC 68, the Eisenhower "New Look," the Kennedy-Johnson "flexible response," and, finally, the concept of *detente*, which motivated the approaches of the Nixon, Ford, and Carter years until the Soviet invasion of Afghanistan in December 1979. Through detailed and studied comparisons of these strategies of containment the author has uncovered their varied patterns and problems.

Kennan, as head of the Policy Planning Staff in 1947, rejected the "universalist" approach of previous administrations, anchored to peaceful procedures, and assumed that change, whether desirable or not, would flow generally from the use of force. To protect the peace against major assaults on the status quo, Kennan favored an equilibrium among the powers. What mattered to him was the protection of those areas of the world that comprised the

equilibrium—the United States, Great Britain, Western Europe, and Japan. For Kennan the Soviet threat was sufficiently limited to permit a policy of containment based on Europe's economic and political rehabilitation. He questioned the lasting relationship between the Kremlin and Communists outside the Soviet sphere; he harbored no fear of free Communist regimes. In competition with the country's burgeoning assumptions of global danger, Kennan's moderate proposals had no chance. To record its perceptions of limitless Soviet designs the Truman administration prepared and adopted NSC 68. That document, submitted to the president in April 1950, acknowledged no distinctions between military and other assaults on the status quo or between regions threatened by communism. To meet the challenge of "frustrating the Kremlin design" the United States required an expanded defense structure, financed by an inflated economy. NSC 68 assumed the danger of war but anticipated an eventual victory through the corrosive effect of containment on the Soviet system. The assumption of victory rendered negotiations irrelevant. NSC 68, by viewing all Communists as equally dangerous, substituted for Kennan's asymmetrical approach to the Communist danger the concept of symmetry.

Eisenhower's New Look accepted the notion of a global danger but sought a less expensive defense strategy to protect the nation's farflung commitments, one based on the threat of massive retaliation. Possessing a strategy aimed at holding the line everywhere without the necessity of war, the new administration faced no greater need to distinguish among interests and goals than did the proponents of NSC 68. The assumption of universal control enabled Secretary of State John Foster Dulles to advocate an asymmetrical strategy. If there was some cohesion in the Eisenhower strategy, as the author suggests, there was never a careful calculation of ends and means. With all his concern for budgetary restraint, Eisenhower did not challenge the concept of falling dominoes or the country's global commitment to protect the entire world against any Communist-led assault. John F. Kennedy and his advisers shared the Eisenhower-Dulles fears that the world balance was precarious and that any further loss to communism would be disastrous. Unlike Eisenhower, Kennedy favored a symmetrical strategy based on larger conventional forces, increased expenditures, and an expanding economy. For the Kennedy administration—and that of Lyndon Johnson—there would be no acceptance of limits to American power and purpose. The Democratic administrations recognized fragmentation within the Communist world, but they refused to base policy on that recognition. The United States would meet any Communist-led challenge any-

where; Vietnam became the ultimate test of that determination. The author passes severe judgment on the Kennedy-Johnson effort to apply flexible response to Vietnam.

What, to Gaddis, the Nixon-Kissinger leadership sought to achieve was a strategy that combined the tactical flexibility of the Kennedy-Johnson system with the coherence of Eisenhower's, while avoiding the ideological rigidities of Secretary Dulles. For Kissinger the answer lay less in indiscriminate globalism than in the creation of a new international order. Kissinger, like Kennan, would entrust international stability to the world balance of power. By instituting international rules of conduct, detente would bring the Soviet Union and China into the international system as elements of stability. To limit Soviet ambition in the Third World the administration introduced the principle of linkage to assure the application of the rules of conduct everywhere. President Nixon returned to a strategy of asymmetry by reducing the American presence in Asia; yet he engaged in tactical escalations, as in Vietnam, to remind the Soviets that the United States did not hesitate to use force. The principle of linkage proved to be ineffective in determining Soviet behavior in Africa and the Middle East.

This remarkably perceptive study of America's changing Cold War strategies acknowledges the overall success of the United States in maintaining some peace and order in international life. At the same time it exposes the contradictions and the lack of precision that characterized those strategies. At the end Gaddis makes no effort to resolve the fundamental problem that his analysis poses: the failure of the United States to discover a viable balance between its symmetrical and asymmetrical approaches. The shifts from symmetry to asymmetry never applied to Europe, but only to the Third World, and even there they embraced only variations in the definition of means, not of ends. Kennan's containment strategy avoided any commitment to anticommunism in the Third World. But every administration from Truman to Nixon, whatever the means it regarded adequate, accepted the notion that the USSR, through its encouragement of subversion and guerrilla warfare, could challenge the non-Soviet world in regions and on issues that far transcended the reach of Russian armies. Assuming the existence of global danger, each administration in turn recommitted the country to the resistance of communism in all of its forms. Thus preferences for symmetry or asymmetry reflected varying attitudes toward the federal budget and varying preceptions of the immediacy of Third World issues but never varying notions of national obligation. In the long run strategy cannot be divorced from commitment; this rendered the differ-

ence between symmetry and asymmetry largely irrelevant. The country would resolve the question of strategy when it defined with greater precision the nature of the challenges that it faced.

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DAN CALDWELL. *American-Soviet Relations from 1947 to the Nixon-Kissinger Grand Design*. (Contributions in Political Science, number 61.) Westport, Conn.: Greenwood Press. 1981. Pp. xiv, 283. \$27.50.

This is a most important book on the development of Cold War strategies and responses, in part because it gathers in one volume the evidence of the substance and aims of the Soviet and American leaders, plus it traces in detail their path to detente. Dan Caldwell divides the 1947-76 phases of the Cold War into three periods: the acute Cold War (1947-62), limited detente (1963-68), and the detente periods (1969-76), with complete descriptions of the whys and hows of transitions that led from one to the other.

By defending the rules, explicit and implicit, that the antagonists developed in their relations on various competitive levels, the procedural evolution of the Cold War is explicated. Also, Caldwell lays out each step in the process of accommodation and conflict resolution leading up to "the Nixon-Kissinger Grand Design and Grand Strategy" that encompassed the detente era.

If a problem exists with the book, for some readers it will be the "bi-lingual" approach. The introduction is written in the language of the behaviorists and quantifiers, delineating "regimes" that "are the norms procedures, and/or organizations created to manage the activities of international actors within issues areas in which there are mutually perceived needs for cooperation to regulate existing conflicts of interest or to promote some mutual interest" (p. 4). If one does not remember this explanation of a "regime" when the word is used later, the remainder of the text will be confusing. Also, one must recall the four characteristics of a "regime" outlined on page 5. Thus when the reader gets to the second language—English—(which, fortunately, is most of the book) one must keep in mind one and one-half pages of definition to comprehend sentences where "regime" is used, for example, "The chronic problems of the Soviet economy in general and the agricultural and advanced technology sectors in particular motivated Soviet leaders more than U.S. leaders to develop a comprehensive economic regime" (p. 198). Readers must pay equal attention to "issues areas," which have special definitions, and "systems."

Caldwell's technique in structuring the book is pure pedagogy; each major thesis is stated, restated, and summarized, and all subtheses are enumerated. In a study that assesses a highly complicated process of precedents and developments this is an effective device.

Most important are the conclusions that establish the concern of the Russian and American leaders for the nearly disastrous collisions of the acute Cold War that led to tentative and then more precisely defined agreements on crisis management, SALT, economic policies, and delineations of vital spheres of influence. Extensive case studies prove these areas of mutual interest. The place of public opinion in limiting foreign policy management both in Russia and America gets deserved attention. And, finally, the breakdown of detente gives a chilling prospect for an uncertain future due in part to misperceptions of the major actors and the internal orientation and amateurism of the post-Nixon administrations. Because of its objective, realistic analysis of detente this is must reading for Soviet-American scholars.

EDWARD M. BENNETT
Washington State University

ROBERT A. DIVINE, editor. *Exploring the Johnson Years*. Austin: University of Texas Press. 1981. Pp. vii, 280. \$25.00.

EMMETTE S. REDFORD and MARLAN BLISSETT. *Organizing the Executive Branch: The Johnson Presidency*. (An Administrative History of the Johnson Presidency.) Chicago: University of Chicago Press. 1981. Pp. x, 277. \$21.00.

Comparisons do not abound between these two works under review, although both are concerned entirely with a single presidency and both rely heavily on material found in the Johnson presidential library. Emmette S. Redford and Marlan Blissett are concerned with structure and process in certain areas of the executive branch. *Exploring the Johnson Years*, on the other hand, is a collection of separately authored monographs treating certain facets of the Johnson era analytically and historiographically. The latter work is more the stuff of history; the former will hold more interest for the political scientist.

Organizing the Executive Branch carefully scrutinizes three major agencies: Housing and Urban Development, Department of Transportation, and the Office of Economic Opportunity. Later sections ("Protecting the President's Hierarchical Structure" and "An Executive Guidance and Coordination System") are very thoughtful analyses of the tribulations of management at such a heady level.

This book is the initial offering in a twelve-volume undertaking with funding from the National Endowment for the Humanities and other sources, under the general title, *An Administrative History of the Johnson Presidency*. Should the project be seen through to completion, and if it maintains the level of objectivity and scholarship displayed by Redford and Blissett, it will obtain a unique place in the ongoing study of the American presidency: not only will the series be indispensable to students of the Johnson era, it will be of equal value to those scholars examining the awesome managerial process involved in the executive function of the American presidency.

A basic rule well known to physicists and editors is that the whole is equal to the sum of its parts. In the case of Robert Divine's *Exploring the Johnson Years*, the parts—a survey of the Johnson literature by the editor and seven other essays on everything from the War on Poverty to the war in Vietnam—form a most agreeable whole.

Although the original intent of these essays was to describe the holdings in the LBJ library at Austin, it would be a serious error to dismiss this book as little more than an advertisement and general catalogue of a presidential library. Most of these monographs provide valuable insights, all are solid pieces of scholarship, and a few are particularly deserving of attention. Among the more noteworthy is George C. Herring's "The War in Vietnam."

Herring reviews the domestic struggle between the hawks and doves and the difficulties this created for an administration seeking an honorable exit from the Indochinese peninsula. He does believe LBJ was quite sincere in his avowed willingness to accept a negotiated settlement, but his position was too rigid and his efforts inept at best. As did Eric Goldman before him, the author portrays Johnson as a "tragic figure," a man, Herring says, "trapped in a dilemma not entirely of his own making and stubbornly persisting despite the enormous pain he was inflicting on the nation and himself" (p. 55).

Among the others, the editor's introduction, entitled, "The Johnson Literature," stands out as a gem of historiography, surveying the thin menu of offerings on Johnson to date with wit and insight. Walter LaFeber's essay, "Latin American Policy" does no disservice to his fine reputation. LaFeber believes that the president suffered from a simplistic view of anticommunism and that his Latin policy, "... with the notable exception of the counterinsurgency program, had not functioned well" (p. 85). Lastly, David Culbert's "Johnson and the Media" displays the fabled LBJ ego in full flower. An earlier work by Doris Kearns, *Lyndon Johnson and the American Dream* (1976) had touched upon LBJ's terrible frustration at his inability to "handle" reporters. One of the

effects of the Culbert essay is to amplify and document Johnson's obsession with his media image and his stillborn efforts to project a more favorable view of himself and his administration. The article is profusely illustrated with archival photographs of great interest.

Other than those noted above, the subjects examined are civil rights, federal education policy, and White House staff relations with the president.

Exploring the Johnson Years is a worthy effort by Divine and his collaborators. It will be mined by avid young doctoral candidates seeking dissertation topics for some time to come.

RICHARD F. HAYNES
Northeast Louisiana University

CANADA

JOHN G. REID. *Acadia, Maine, and New Scotland: Marginal Colonies in the Seventeenth Century*. Buffalo: University of Toronto Press. 1981. Pp. xviii, 293. \$30.00.

With this examination of "marginal" colonies in what he calls the "northeastern maritime region," John G. Reid has made a most important contribution to the inadequately developed field of comparative colonial studies. Acadia, Maine, and New Scotland—later Nova Scotia—were contiguous, in fact overlapping, colonies. Each was sponsored by a different European crown, each involved the interaction of European and native cultures and the interaction of a separate European culture with the natural American environment, and all were affected by their relationships with one another. By the end of the seventeenth century, not only had each of the colonies failed badly to realize its founders' expectations, but also each in a different way was a scarcely recognizable shell of its original self. Intercolonial warfare and imperial politics had combined to reduce Acadia to a single military camp on the St. John River, to re-establish a Nova Scotia then recognized only by Massachusetts, and to empty Maine of all settlements east of Wells while in the status of a county of Massachusetts instead of a proprietary province.

The failure of all three colonies in the seventeenth century Reid attributes to their "marginal" status—marginal in the sense that they were all on the edge of European settlements, that they all lacked a solid political and economic basis on which to support a community of settlers, and that they were, in Reid's words, "dependent upon external forces for their characteristics and their very existence" (p. xv).

But though failure and "marginality" constitute

one theme of Reid's study, there are other themes as well. One is the consistent gap between the American actualities and the European conceptualization of America as seen in these three colonies. Another is the constant interplay between the native American and the three European cultures. A third, unstated but imbedded in the organizational scheme of Reid's compact study, is the parallel lines of development and dissolution in the three colonies, traced here in chapters defined primarily by chronological segment.

This book, like Reid's first-born, *Maine, Charles II and Massachusetts* (1977), emphasizes political and institutional matters and uses the historian's traditional sources. As in his earlier effort, Reid uses such sources—the manuscript public records of four nations and printed primary sources in two languages—extremely well. In addition to exploiting a rich blend of primary sources, Reid has erected a conceptual framework that is both illuminating and ingenious, in the process making use of the perspectives of recent historical, geographical, and anthropological scholarship, most notably that involving the American Indian.

Reid will no doubt be criticized, and with some justification, for having failed to do much by way of social history or with life at the level of family and locality. Both these perspectives have obvious potential for comparative studies, but Reid concentrates primarily upon disclosing imperial, intergovernmental, and intragovernmental relationships in a geographically united but politically divided colonial region. One might argue with his choice of emphases, but for what he chose to do, the traditional methods and traditional sources serve him better than those, for example, of the new social history. And as already demonstrated in his earlier book, these are very much Reid's strong suits.

CHARLES E. CLARK
University of New Hampshire

PATRICK A. DUNAE. *Gentlemen Emigrants: From the British Public Schools to the Canadian Frontier*. Vancouver, Canada: Douglas and McIntyre. 1981. Pp. vii, 276. \$16.95.

Recently the work of migration historians has manifested two important trends. These historians have focused their attention on specific populations, and they have borrowed the methodologies and explanation forms of other social scientists. Patrick A. Dunae's *Gentlemen Emigrants* reflects the first but not the second of these trends. Instead, the book maintains the genteel tradition of imperial history associated with sherry and Oxbridge common rooms.

Dunae treats "the younger son question" in a chronological narrative that takes the redundant

scions of Britain's middle-class and upper-class families from the back concessions of Upper Canada to the rose gardens of Victoria. Undeniably anecdotes about the immigrants' experience in rural Canada have a certain charm. Unfortunately, the narrative also has the impressionistic quality of a series of vignettes. Accounts of the Ashcroft hunt depict an important cultural form that British gentlemen introduced to the West. But surely the observation that "the bonds formed over stirrup cups were often reinforced by marriage" should also have been explored. Did these patrician immigrants practice preferential endogamy to maintain family and class networks? Anglican parish records permit analysis of such questions. Even more disappointing, given the focus of the book, is Dunae's uncritical rehearsal of the "remittance man" caricature. He does not attempt to determine whether younger sons were, in fact, more exclusive and less abstemious than other Europeans. Deportation records would throw some light on the question. Anecdotes never become cases in support of an argument, because Dunae's discussion does not develop within an analytical framework. The author is clearly aware of the collective assertion of British ruling-class culture in Canada. He appears uncertain, though, whether to treat this ascription as conventional immigrant behavior or as an impediment to accommodation. Since Dunae's purpose is partly to demonstrate that his immigrants "were members of a distinct ethnic group," behavior and institutions must be analyzed, not merely described.

The deficiencies of the book result in part from its sources. Dunae has relied almost exclusively on memoirs and travel accounts. Although the catalogue of these works is impressive, the commercial and didactic purposes of such material limit its application. Sources exist in Britain that elaborate the mundane experience of younger sons in Canada, for instance, the Marvin Papers in the Bodleian Library. But the most serious deficiency in Dunae's sources is his failure to consult the files of the Immigration Branch. For more than a decade historians have recognized that Canadian immigration studies simply cannot be written without those records.

In many ways, the "supernumerary gentlemen" were charming anachronisms in late Victorian and Edwardian society. The book that Dunae has written about them is also a charming anachronism.

A. ROSS MCCORMACK
University of Winnipeg

LATIN AMERICA

FERNANDO PICÓ. *Amargo café: Los pequeños y medianos caficultores de Utuado en la segunda mitad del siglo XIX*. Río Piedras, Puerto Rico: Huracán. 1981. Pp. 162.

The cultivation of coffee was introduced into Puerto Rico in the 1720s and by the second half of the nineteenth century coffee had replaced sugar as the island's most important export. Whereas sugar cultivation was concentrated along the coastal plains, the cultivation of coffee was centered in the highlands of the interior (the *cordillera central*) where, by the 1890s, the municipality of Utuado had emerged as one of the key coffee-producing areas and was demographically the second most important municipality in Puerto Rico.

The economy and society of interior municipalities like Utuado were far more complex than has been pictured by historians until recently. It was a society in which large landowners (*hacendados*), small and medium-sized farmers, merchants, and landless workers (*peones*, *jornaleros*, and the like) interacted with each other in ways that remain to be explored in greater detail.

In his first book, *Libertad y servidumbre en el Puerto Rico del siglo XIX* (1979), Fernando Picó of the University of Puerto Rico dealt with the landless rural work force in the Utuado region in the third quarter of the nineteenth century. In *Amargo café* he focuses on the small (*pequeños*) and medium-size (*medianos*) coffee growers of the municipality from the 1870s when coffee production underwent impressive growth until the crisis in the coffee economy brought on by the U.S. blockade and invasion of 1898. In part 1 (chaps. 1–5) Picó outlines the various phases the Puerto Rican coffee economy went through between 1870 and 1914, how land was acquired and passed on, the relations between small and medium-sized producers and the merchants who provided them with badly needed credit, and the nature of the labor force in small and medium-sized holdings. Part 2 (chaps. 6–9) consists of a series of brief family biographies that provide the reader with useful information on some of the small and medium-sized producers of Utuado: their background, family size, life span, and economic vicissitudes. The conclusion recapitulates the major themes of the book and makes some interesting comparisons between the small and medium-sized producers of Utuado and their counterparts in Mexico's Bajío region.

Using parish and other archival documentation, Picó has produced a readable and well-organized account of a group of producers whose members were not, as has been often argued, rapidly falling into the ranks of the rural proletariat. Rather, they constituted a stable socioeconomic group that by the end of the century accounted for close to 60 percent of coffee production in the municipality and were often better able than the larger producers to weather downturns in the coffee economy. Although the author does not define clearly the variety of property-related terms (*haciendas*, *latifundio*, *hatos*, *estancias*, *grandes propietarios*, and so forth) used in the book,

the work is on the whole a fine introduction to the study of the society and economy of the coffee-producing areas of Puerto Rico in the nineteenth century and constitutes an important contribution to Puerto Rican historiography.

ADALBERTO LÓPEZ
State University of New York,
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PETER EARLE. *The Sack of Panamá: Sir Henry Morgan's Adventures on the Spanish Main*. New York: Viking Press. 1981. Pp. 304. \$16.95.

A swashbuckling, rollicking tale of sailing ships and derring-do, complete with haughty dons, wily English governors, and scruffy but intrepid privateers—not that moldy, romanticized, overdone, and half-baked gang on the Spanish Main again. Fortunately this rehashing of the Morgan era in the Caribbean has several redeeming features and ends up as a genuine if minor contribution.

First, this book is remarkably well written, as this kind of narrative tale of well-known events must be if it is to survive. Earle carries the tale forward, ingeniously switches scenes and personalities, describes persons and places with considerable dexterity, and leaves the reader well satisfied.

Second, Earle has done extensive research. He has been through Alexander Exquemeling, the other contemporary writers, and the familiar English sources such as the manuscript division of the British Library and the Public Record Office. He has also done considerable digging in Spanish sources in the *Archivo General de Indias* in Seville, especially in the *averiguaciones* and *residencias* of the *Escribanía de Cámara* section. The result is an even-handed, careful account that reports more of the Spanish side and eliminates some of the grosser exaggerations about numbers of people and heroic deeds.

The book is more than the title promises. The author begins with Edward Mansfield's capture of Providence in 1666, the Spanish recapture the same year, Henry Morgan's sack of Portobello and Maracaibo, and their repercussions in Jamaica and Europe, before arriving at the main business, Morgan's attack on Chargres, his advance up the river and across the isthmus, the feeble defense of Panama by Juan Pérez de Guzmán, one of Earle's favorites, the battle outside the city, and the burning and sack of the settlement and region.

The author concludes with an analysis of the end of the era of privateering and the emergence of a planter society in Jamaica. The pillagers settle down or leave for other shores, and, Earle suggests, duller men and times take over.

The reader may not agree with the author's preferences and opinions, but the book is a fresh,

thorough, and readable version of an old tale, which is surely more than enough.

MURDO J. MACLEOD
University of Arizona

KIERAN MCCARTY. *A Spanish Frontier in the Enlightened Age: Franciscan Beginnings in Sonora and Arizona, 1767–1770*. (Publications of the Academy of American Franciscan History, Monograph Series, number 13.) Washington: Academy of American Franciscan History. 1981. Pp. 116. \$20.00.

EVELYN HU-DEHART. *Missionaries, Miners, and Indians: Spanish Contact with the Yaqui Nation of Northwestern New Spain, 1533–1820*. Tucson: University of Arizona Press. 1981. Pp. viii, 152. Cloth \$19.95, paper \$9.95.

Despite extremely imprecise and excessively inclusive dates of 1533 to 1800 in the subtitle of *Missionaries, Miners and Indians*, and very restrictive dates of 1767–70 for *A Spanish Frontier in the Enlightened Age*, the contents of the two books form a sequence. Except for length, just over one hundred pages each, there are few similarities. One notable exception is that both have clear maps done by the area's expert mapmaker, Don Bufkin, who provides indispensable cartographic representations, as he has done for previous studies of the region.

A disparity of approach and result is notable in several ways, almost always to the advantage of Kieran McCarty's volume, which is set in letter press of clear-faced and almost error free typography. The advantage of clarity of writing style and felicity of expression likewise rests with the Franciscan study done by McCarty rather than Evelyn Hu-DeHart's work on the earlier Jesuit period. The few minor errors of the former contrast with the more abundant mistakes of the latter. The preciseness of expression, the good maps, and general format of the *Spanish Frontier* book are commendable.

The Jesuit-oriented paperbound book frequently reveals Hu-DeHart's inexperience in both syntactic and factual mistakes. There are also too many mechanical errors for a brief book. A short background is followed by a chapter on the entry of the Jesuits into Sinaloa and Sonora beginning in the late sixteenth century. With ample military assistance from frontier army captain Diego Martínez de Hurdaide, the Jesuits were able to penetrate Yaquiland with the lower course of the Yaqui River soon under missionization at Cócorit, Bâcum, Tórin, Vicam, Pótam, Ráum, Huiviris, and Belém.

The Sonora Jesuits were not without their detractors and active opponents, but the long struggle between priests who had been given wide latitude in their missionary activities and the local citizenry came into sharp focus at the time of the Yaqui revolt

of 1740. The priests became targets of attack on the issue of control of native labor, on matters of temporal jurisdiction, and because of Indian-instituted desire for major changes in their social status. The regional miners, very important to economic development of the northern frontier, were in definite conflict with Jesuit plans for conversion and civilization of the Yaqui, a training regimen that deprived the miners of a potentially docile and underpaid work force.

The Hu-DeHart study concludes with the Jesuit expulsion of 1767, the low point of long and heroic efforts by scores of black robes dedicated to evangelization. The expulsion is treated in standard fashion including the central role of Visitor General José de Gálvez. It was his responsibility to carry out the purge, a job he undertook with enthusiasm. The expulsion resulted in a less drastic change in Yaqui life than might have been predicted.

A *Spanish Frontier in the Enlightened Age*, a hard-bound book, takes up where the Hu-DeHart study leaves off, with the Jesuit expulsion in 1767, and concentrates on the early years of the Franciscan replacements and their ministry in Sonora-Arizona. McCarty consequently deals with an area just north of the Yaqui lands treated in the other book. The mood for the study is satisfyingly established in the frontispiece with an excellent artistic reproduction of a characteristic Franciscan frontier missionary done by the Borderlands' top artist, José Cisneros. The subsequent narrative takes up at the very point of Jesuit exodus and deals with Franciscan participation in "Operation Replacement," a commitment already agreed to by the Franciscans prior to promulgation of the Jesuit banishment edict. The frontier-bound grey robes united at Tepic for a trip to Matanchel on the Pacific, from which point they set sail toward the "Northern Mystery" as McCarty calls the unknown land to the north. Rapid occupation of the leaderless former Jesuit missions was the immediate goal, one quickly but not always easily attained because of a jockeying for position and leadership between the order priests and the diocesan clergy. The final chapters describe the physical setting and the progress of early work among the Pimas, a section useful for a better understanding of the changed frontier pattern.

DONALD C. CUTTER
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ERIC VAN YOUNG. *Hacienda and Market in Eighteenth-Century Mexico: The Rural Economy of the Guadalajara Region, 1675-1820*. Berkeley and Los Angeles: University of California Press. 1981. Pp. xvi, 388. \$30.00.

Historians investigating colonial Latin American society have recently been concentrating on the rural economy, its structure, and relationship to urban, regional, and intercolony factors. Eric Van Young's carefully crafted book about the Guadalajara region reflects this trend in terms of time period, types of sources utilized (secular and ecclesiastical archives), research techniques (the incorporation of economic, demographic, and urban-development theoretical insights), and geographical area (colonial Mexico, which has been getting the lion's share of scholarly attention). Such activity has resulted in strikingly differing interpretations of colonial society—feudal, precapitalist, capitalist—and simultaneous publications in Europe and the Americas by specialists within the same field of study. Van Young's evidence and conclusions indicate significant capitalist trends in the eighteenth century, and his innovative incorporation of diverse theoretical ideas allows him to transcend the too-often dogmatic feudal-capitalist debate and to illuminate the process of societal change.

The book is divided, thematically, into four parts, amply supported by illustrative documentation (21 figures and 29 tables). The author clearly identifies self-imposed geographical and other limits. His dual objective is to provide a structural analysis, demonstrating how the region became internally integrated over time, and to show how population increase within Guadalajara City resulted in the development of commercial agriculture in surrounding rural areas. His emphasis is clearly on the rural economy rather than rural society as such, the nature of which—including small-scale farmers and ranchers, peasant villages, and rural-urban middlemen—is consciously omitted from the analysis. These omissions, justifiable in light of lack of archival information and the specific intent of the study, nevertheless force the reader to take a "leap of faith" in accepting conclusions relative to impact on these elements of rural society.

The first three parts of the book add greatly to our stock of demographic and economic knowledge about the Guadalajara region. Part 1, which provides an overview of the physical and human context of colonial society, and part 2, with chapters on meat, wheat, and maize, identify economic and demographic factors of regional rural-urban interaction. The stress is on the impact of urban population increase (sixfold with a century), the resulting expanded regional market, and the variety of monopolistic policies that attempted to control price and supply. The weight of the study, however, is in part 3 (46 percent of the text), which focuses on the rural haciendas. Their ownership, sources, and patterns of capital investment, production strategies, labor, and technological utilization are described in detail. And although Van Young finds little that has

not been found to hold true by other studies of other regions of Mexico, he successfully documents much-needed details for a previously understudied region. His six chapters in this section clearly show—and in so doing place another nail in the coffin holding the remains of earlier perspectives regarding the dominance of a self-sufficient, encapsulated, seigneurial domain—a regional expression of agrarian enterprise owned and operated in the pursuit of profit.

In part 4 the emphasis shifts to the implications of population pressures, conflicts over land, and the nature of internal regional economic integration. Van Young shows how investment and labor policies failed to stimulate significant technological innovations yet resulted in increased and more specialized rural production, leading to increased profits for landowners and greater status and wealth differentiation in both rural and urban sectors. His speculations about the significance of a regional study for understanding the process of societal change in other areas should stimulate further debate and a better understanding about specific issues, such as the proletarianization of the peasant sector and the impact of population pressures. As a case study the book confirms much of what has recently been documented for other areas of Mexico while adding significant new details. In terms of technique and ideas it is an important contribution to the field of colonial economic and social history.

HERMAN W. KONRAD
University of Calgary

GEORGE A. BOWDLER and PATRICK COTTER. *Voter Participation in Central America, 1954–1981: An Exploration*. Washington: University Press of America. 1982. Pp. xi, 262. Cloth \$21.75, paper \$11.00.

This recent offering of the University Press of America reflects both the strengths and weaknesses of that innovative venture in academic publishing. On the one hand, it is a timely volume on a topic of substantial contemporary interest produced in an economical edition. On the other, it suffers from careless preparation and lack of editorial refinement. The text is in places barely legible and innocent throughout of editing for style and grammar. Awkward, impenetrable sentences are made more formidable by the author's unfortunate imprecision in the use of terms. The organization of material within chapters appears hurried and arbitrary. Moreover, the work's utility is greatly diminished by the absence of a bibliography and an index.

The purpose of the study, according to George A. Bowdler, its principal author, is "to interpret the complexities and various meanings of electoral participation in Central America to students and read-

ers who may know little about the region" (p. iv). In reality, the first five chapters are devoted instead to a descriptive survey of the recent political history of each of the five historic Central American republics—Guatemala, El Salvador, Honduras, Nicaragua, and Costa Rica. The sixth and final chapter, labeled "Conclusion: Insights from Empirical Explorations," presents the results of a questionnaire (not reproduced) as well as of a correlation and regression analysis of poorly defined data whose relationship to the body of the work is never clearly stated. The conclusions appear to be obvious ones, among them that elections have been most free and meaningful in Costa Rica and least so in prerevolutionary Nicaragua. The assertion that "the other Central Americans . . . simply do not want to imitate the Costa Ricans" has little explanatory force (p. 204).

The chapters devoted to political overview are informative, but they are flawed by vague chronology as well as occasional errors of fact. For example, the presidential candidate of the Unión Nacional Opositora in El Salvador in 1977 is incorrectly identified as "a Colonel Carmún" (p. 7). Some statements are simply mystifying. Guatemalan Mario Méndez Montenegro is described ten pages apart as having been at the time of his assassination both "a conservative centrist on the right" (p. 129) and a "left of center political leader" (p. 139). Bowdler's chief sources for his discussion of each country are interviews and newspaper accounts. Unfortunately, he tends to accept uncritically other observers' value judgments as well as their at times faulty recollections. Curiously absent is any significant reference to the extensive body of existing scholarship on Central American politics. There are at least a dozen writers not cited by Bowdler whose works he might have consulted with advantage.

In a year when the outcome of Central American elections is perceived as consequential to United States policy, there is a need for a general work on the subject. The present study, however, offers little new to the specialist, and the nonspecialist is well advised to approach it with caution.

STEPHEN WEBRE
Louisiana Tech University

JUDITH EWELL. *The Indictment of a Dictator: The Extradition and Trial of Marcos Pérez Jiménez*. College Station: Texas A&M University Press. 1981. Pp. viii, 203. \$18.50.

Judith Ewell's *The Indictment of a Dictator* is an intriguing book. It describes the extradition and trial of former Venezuelan strongman-president Marcos Pérez Jiménez, a process beginning with his

overthrow and flight to the United States in 1958 and ending ten years later with his conviction for illicit enrichment by a Venezuelan court. Ewell's account of this process is in itself an informative study of international law and jurisprudence. She takes the case beyond the courtroom, however, and places it in a broader historical and political context, where, indeed, it belongs.

Ewell presents the trial of Pérez Jiménez as the last in a series of clashes between the dictator and his democratic rival, Rómulo Betancourt. In this way, she enlarges the scope of the book, tracing the careers of the two men and comparing the philosophy and nature of their respective governments. In this last confrontation, with Pérez Jiménez a defeated man and Betancourt in the presidency, one might assume that Betancourt had the advantage, but, as events unfolded, Pérez Jiménez almost staged a comeback, and Betancourt and his government, in effect, went on trial.

This turnabout stemmed from the fact that Pérez Jiménez could not be tried in the courts for being a dictator. The conditions of his extradition from the United States permitted him to be tried only for the specific offenses of embezzlement, speculation, and malversation. As a president, Pérez Jiménez had been accused of terrible crimes, but, as a defendant, the Venezuelan prosecution had difficulty proving that he was a common criminal. Under the circumstances, Pérez Jiménez perceived the opportunity to discredit everything that had been said about him and launched into a vigorous defense of his conduct and government. Although this defense was irrelevant to the charges, Pérez Jiménez was convicted only of the minor offense of continuous enrichment in office and received a light sentence, which was satisfied by the time he had already spent in jail, permitting him to go free and creating the impression that he had been exonerated of all crimes. Had he been more astute, Ewell writes, he might have renewed his political career.

After reading this book, one is tempted to conclude that it was a mistake to try Pérez Jiménez. The judicial system did not lend itself well to what was essentially a political action. On the other hand, considering that Pérez Jiménez spent nearly ten years in courtrooms and jails, exhausting his energy and resources, one might suggest that he paid for his abuse of power. This is a thought-provoking book, enhanced by the fact that it is carefully researched and well written.

CHARLES D. AMERINGER
Pennsylvania State University

ROSE MARIE BUECHLER. *The Mining Society of Potosí, 1776–1810*. (Dellplain Latin American Studies, number 7.) Ann Arbor, Mich.: University Micro-

films International, for the Department of Geography, Syracuse University, Syracuse, N.Y. 1981. Pp. xv, 431. \$24.50.

The late eighteenth century marks the apogee of a conscious attempt on the part of the Spanish crown to revitalize the economies of its American colonies. First in Mexico, and then in Peru, part of this economic reform included attempts to increase mineral production and augment the resultant profits (in the form of taxes) for the royal coffers. By the 1780s, Mexican silver mines, the initial focus of royal interest, had begun to outstrip the production of Alto Perú. The government's response to the growing inequality between Mexican and Upper Peruvian production was to attempt recasting the system of mineral production of the "Imperial City" of Potosí, along proven Mexican lines. The subsequent history of this many-faceted reform program is the subject of this book.

Drawing on wide-ranging sources from archives in Spain and America, Rose Marie Buechler weaves the complicated strands of royal policy, mining interests, Indian labor, and government bureaucrats and agencies into a compelling story of how and why reform failed in Potosí. Against a warp of a slow-moving and complex bureaucracy, local interests, and personal vendettas, Buechler skillfully inlays the woof of the Caroline Mining Code, the Nordenflicht expedition, the regime of Intendent Francisco Paula Sanz, mercury shortages, and natural disaster. The resultant fabric (to continue the analogy just a bit further) demonstrates the degree to which miners' conservatism and intransigence diffused government plans to introduce new technologies into Potosí, and instead substituted programs calling for increased conscript labor (Indian *mitayos*) as the key to solving production problems. Furthermore, in the case of Potosí the very Intendent sent to revitalize the mining industry became a spokesman for a faction of the traditional mining sector, pursuing policies that favored a select group of individuals at the expense of the entire mining industry, Upper Peru, and the Spanish empire as a whole.

Although Buechler's treatment of the mining reforms brings new and important insights into the question of the efficacy of the Bourbon reforms in general, and the Potosí mining district in particular, there are several flaws that mar this work. First, the title of the book is somewhat misleading. With the exception of one chapter where she treats a handful of mine owners and renters, reform, not the mining society of Potosí, is the focus of this study. From the introduction it is clear that this study is part of a larger doctoral dissertation that also included sections on trade, agriculture and industry, mercury transports, and the relationship of Potosí to other

urban centers. Without even a synopsis of this research, the reader never appreciates the complex economic interests that in part determined miners' actions and reactions.

While the book focuses on government plans for reform, and the response of the miners to these proposed changes, a more detailed description of the organization of mining, especially the role of less powerful producers (*trapicheros*, *k'ajchas*, *socavoneros*) is needed. More information on the working of the *compañía de azogueros*, and its successor institution, the *banco de rescates* (later to become the Banco de San Carlos) would also have been useful, as would more detailed analysis of the *gremio* system and its relationship to mining in Potosí. In addition, although the author presents figures on the yearly amounts of silver brought to the Banco de San Carlos, she fails to attempt any analysis of production trends or to relate production to the ongoing battle for and against mining reform. Given Buechler's beautiful writing style, these omissions are most unfortunate.

In sum, although weakened by a narrow focus and limited socioeconomic analysis, this study is an important and most readable contribution to a growing literature on the Bourbon reforms in Spanish America.

SUSAN M. SOCOLOW
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EMILIO F. MORAN. *Developing the Amazon*. Bloomington: Indiana University Press. 1981. Pp. xvii, 292. \$22.50.

The vast Amazon plain has excited imagination and greed since the first Europeans entered the area in 1542. In the succeeding four centuries outsiders have taken from the Amazon region Indian slaves, rubber, and other products of the forest. The extractive cycles are often described in "boom and bust" imagery, but from the perspective of the native peoples and the natural environment the successive "booms" have brought little but despoliation and exploitation.

Until the last third of the twentieth century the technology and capital available to the outsiders was no match for the grandeur of the Amazon. Only some 2 percent of the area drained by the river-sea had been effectively occupied by the intruders. Around 1970 the authoritarian government of Brazil resolved to use its recently consolidated political and economic power to make a new assault. The target was again the natural endowment. The new means was a network of access highways to open the interfluvial uplands to extractive activities for the first time. Prominent in the public rhetoric surrounding these new efforts was the program treated in detail in this book: the establishment of farming

communities along the highway system, to be populated by families from other parts of Brazil. In this meticulous, judicious, and solidly grounded study anthropologist Emilio F. Moran examines the rise and fall of the "agrovila" idea of the early 1970s.

Moran establishes the historical, environmental, and socioeconomic context of the colonization effort, then measures the farm colonies against the justifying statements of the government and the ecological characteristics of the area to be settled. The core of the book is a detailed presentation and analysis of voluminous field notes the author collected in one agrovila along the upper Xingu river in 1972-74. His general conclusion is at the same time a truism and a necessary observation: the world is more complex than people might wish it to be.

A clear contrast emerges between the overly optimistic macrolevel view of the planners, predicated on unwarranted assumptions of social and ecological homogeneity in the Amazon and the microlevel specificity of Amazonian ecology and of the colonists themselves. In 1974 the government shifted its emphasis away from small farm colonization in favor of timber, mineral, and livestock operations, reverting to an extractive policy but now on a grand scale. By that time the people of the agrovilas had begun to reconstitute a society by a variety of strategies for dealing with the local ecology, economic and bureaucratic networks, and each other. The microlevel scene was neither one of general success nor unmitigated failure, but a complex story of adaptation.

This book presents an overview and an informative case study of the latest stage of outside intrusion into the Amazon, one of the last frontiers of territorial expansion of the world economy. More broadly, it provides telling insights into processes as old as colonization itself.

THOMAS H. HOLLOWAY
Cornell University

MANUEL TORRES MARIN. *Chacabuco y Vergara: Sino y camino del Teniente General Rafael Maroto Yserns*. Santiago: Andres Bello. 1981. Pp. ix, 483.

The 1973 military coup wrecked academic life in Chile. Many of the best young scholars were scattered abroad while those who remained became exceedingly cautious. With a few admirable exceptions the survivors have given themselves over to hero worship and hagiological tracts on defunct generals and right-thinking ministers. These works, sometimes dedicated to the armed forces, are the counterpart of the tomes devoted to workers' struggles that flourished in 1970-73, the heady years of the Popular Unity.

In some 483 pages Manuel Torres Marin follows

the career of a most unlikely figure for adulation; indeed, his engaging and often decorative prose threatens to rescue another general from the obscurity he richly deserves. General Rafael Maroto had an instinctive eye for lost and even unworthy causes. Born in Spain in 1783, he came to Chile in 1814 to combat the movement for independence. He promptly led his Talavera battalion to unnecessary slaughter at Rancagua and then suffered a crushing defeat by the reassembled patriot forces at Chacabuco in 1817, a battle that assured Chilean independence. Between these two engagements Maroto made an uncharacteristically smart move: he allowed himself to be subdued by one of the great natural resources of Chile, its women, and married the nubile heiress to a substantial local fortune. The next several years were passed in desultory and ultimately disastrous combat against the insurgents in Peru. In the end, in 1824, pushed into the sea by Bolívar's triumphal army, Maroto hurried his long-suffering wife and two infants on board a waiting Spanish man-of-war for the journey home. Unimpressed by his heroic sacrifice in the imperial cause, the ship's captain demanded 1000 *duros* for the passage on the spot.

Once returned to Spain, General Maroto was drawn unerringly to the losing side in the civil war. Caught in an early conspiracy he was tried for treason but in time emerged as the leader of the Carlist forces in the north largely on his ability to "electrify" the troops. But the general himself seems to have become lukewarm for combat. In 1839, he embraced his enemy and signed the Convention of Vergara, which recognized Isabella as legitimate queen and also safeguarded the pay of Carlist officers. Forever under a cloud of suspicion and distrust, Maroto returned to Chile to claim his wife's shrinking estate (she, alas, had perished at sea in 1830) and died there in 1853.

Torres Marin follows this largely unsavory story with considerable tenacity and open admiration for the general's military bearing and noble qualities. The subject is clearly not worthy of all this elegant attention. Maroto was a singularly ill-tempered man who punished his mulatta servant girls for the slightest false move, bellowed disastrous orders to his troops in "that hoarse voice of his," and seems to have had truculent quarrels with every superior he served under. In a completely understandable moment of self doubt, the author wonders aloud on page 5 whether it "was really necessary to write a book about Maroto." Recognizing the shaky grounds for a positive response, Torres Marin suggests that Maroto stood for something more important than himself, that he represents in his return to the country whose independence he fought to stifle, a cultural affinity between Spain and Chile that survives the accident of political antipathy. As slen-

der a pretext as this may be for writing a book, there is a more doubtful opportunity he makes use of. The military topic itself permits Torres Marin to point out in the course of his narrative, "the close identification that has always existed between the Chilean people and the Army" (p. 94), and the "enduring spirit" that then as now "has permitted the Army to give its firm support to the country whenever this has become necessary" (p. 302). This, in a most vulgar sense, is what Benedetto Croce meant by the observation that all history is contemporary history.

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Davis

THOMAS C. WRIGHT. *Landowners and Reform in Chile: The Sociedad Nacional de Agricultura, 1919-40*. Urbana: University of Illinois Press. 1982. Pp. xix, 249. \$21.00.

One of the most active and influential political organizations in early twentieth-century Chile was not a party, but a *gremio*, a guild: The National Agricultural Society (Sociedad Nacional de Agricultura, SNA). When the history of the entire century is written with the proper amount of perspective, the SNA along with its industrial counterpart the Society for Industrial Development (Sociedad de Fomento Fabril, SOFOFA), will be seen as significant factors in nearly every major political decision from World War I forward.

Thomas C. Wright's book goes a long way indeed toward providing us with the facts, interpretation, and necessary perspective to revise some ideas on recent Chilean political life. Always known for the resilience of its party system, Chile was for years singled out as a relatively advanced Latin American democracy. Recent events have shown just how fragile the political system was. Wright shows just how significant the SNA really was as representative agent for the interests of agriculturists and those connected to them as they were caught up in the sociopolitical and economic changes of the interwar years.

Seven solid chapters and an incisive epilogue treating the post-1940 decades make a strong case for historical forces at work in recent times; raise a number of questions about the "legitimacy" of political systems; and document clearly the inner workings of the SNA as club, political wing of rural oligarchs, guild, and counterrevolutionary center of corporatist thought.

Throughout, Wright meticulously shows just what the SNA's primary function was by describing the relations of landowners with the rest of the oligarchy. The fact that wealthy landowners have

not always been in agreement with other wealthy Chileans may not be surprising, but it is surely made clearer and more historically significant in these pages than anywhere else. Knowledge that there were pressures exerted from the left, and on occasion from the center, for agrarian reform (in various guises) is not new, but nowhere do the specifics of the landowners' responses receive this kind of treatment.

The SNA obviously has not existed in a vacuum. Wright vividly portrays individuals and describes actions that provided links to politics. He treats thoroughly the realities of struggle over price controls, tariffs, and rural labor reform as well as agrarian reform itself. When, in the epilogue, he shows how the interests of landowners coincided

with those of other property owners when confronted by socialism, he gives more food for thought (and hopefully for scholarship) on the collapse of a system in which landowners had been able to survive rather well despite dramatic transformations in industrial and mining sectors.

This is a solid institutional history, precisely defined, well documented, and convincingly written. The book's shortcomings—imprecise use of terms like "upper class," an ineffective comparison of Chilean landowners with North American farmers—are minor. When others write on modern Chile or agrarian-political questions in Latin America, Wright's approaches will prove influential.

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Collected Essays

These volumes, recently received in the AHR office, do not lend themselves readily to unified reviews; the contents are therefore listed. Other similar volumes that are amenable to reviewing will be found in the review section.

SEYMOUR DRESCHER *et al.*, editors. *Political Symbolism in Modern Europe: Essays in Honor of George L. Mosse*. New Brunswick, N.J.: Transaction Books. 1982. Pp. vii, 310. \$49.95.

SEYMOUR DRESCHER *et al.*, George Mosse and Political Symbolism. ROBERT A. NYE, Degeneration and the Medical Model of Cultural Crisis in the French *Belle Époque*. ANSON RABINBACH, The Body without Fatigue: A Nineteenth-Century Utopia. TIM KECK, Practical Reason in Wilhelmian Germany: Marburg Neo-Kantian Thought in Popular Culture. STEVEN E. ASCHHEIM, Caftan and Cravat: The *Ostjude* as a Cultural Symbol in the Development of German Anti-Semitism. DAVID GROSS, Myth and Symbol in Georges Sorel. RICHARD ALLEN SOLOWAY, Feminism, Fertility, and Eugenics in Victorian and Edwardian England. ALFRED H. KELLY, Darwinism and the Working Class in Wilhelmian Germany. H. G. KOENIGSBERGER, Science and Religion in Early Modern Europe. JOAN WALLACH SCOTT, Popular Theater and Socialism in Late Nineteenth-Century France. JOST HERMAND, Dashed Hopes: On the Painting of the Wars of Liberation. PAUL F. LACHANCE, The Nature and Function of Generational Discourse in France on the Eve of World War I. ROBERT A. POIS, Man in the Natural World: Some Implications of the National-Socialist Religion. STERLING FISHMAN, GLM: An Appreciation. PAUL BREINES, With George Mosse in the 1960s.

TIMOTHY TRAVERS and CHRISTON ARCHER, editor. *Men at War: Politics, Technology, and Innovation in the Twentieth Century*. Chicago: Precedent. 1982. Pp. 228.

DESMOND MORTON, Exerting Control: The Development of Canadian Authority over the Canadian Expeditionary Force, 1914–1919. MARTIN KITCHEN, Hindenburg, Ludendorff, and the Crisis of German Society, 1916–1918. REGINALD ROY, Mutiny in the Mountains: The Terrace, British Columbia Incident. DOMINICK GRAHAM, Sans Doctrine:

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ABRAHAM J. PECK, editor. *Jews and Christians after the Holocaust*. Foreword by ELIE WIESEL. Philadelphia: Fortress. 1982. Pp. xvi, 111. \$8.95.

ALFRED GOTTSCHALK, Religion in a Post-Holocaust World. YAFFA ELIACH, Defining the Holocaust: Perspectives of a Jewish Historian. ROSEMARY RADFORD RUETHER, Christology and Jewish-Christian Relations. JOHN S. CONWAY, The German Church Struggle and Its Aftermath. ALLAN R. BROCKWAY, Religious Values after the Holocaust: A Protestant View. IRVING GREENBERG, Religious Values after the Holocaust: A Jewish View. DAVID TRACY, Religious Values after the Holocaust: A Catholic View.

THOMAS T. HAMMOND, editor. *Witnesses to the Origins of the Cold War*. (Publications on Russia and Eastern Europe of the School of International Studies, number 10.) Seattle: University of Washington Press. 1982. Pp. 318. \$22.50.

THOMAS T. HAMMOND, The Great Debate over the Origins of the Cold War. GEORGE F. KENNAN, The View from Russia. MICHAEL B. PETROVICH, The View from Yugoslavia. CYRIL E. BLACK, The View from Bulgaria. WILLIAM HARDY MCNEILL, The View from Greece. CORTLANDT V. R. SCHUYLER, The View from Romania. MARTIN F. HERZ, The View from Austria. LOUIS MARK, JR., The View from Hungary. JOHN A. ARMITAGE, The View from Czechoslovakia. KARL MAUTNER, The View from Germany. RICHARD T. DAVIES, The View from Poland. THOMAS T. HAMMOND, America, Russia, and Eastern Europe, 1941–46.

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PETER CLEMOES *et al.*, editors. *Anglo-Saxon England*. Volume 10. New York: Cambridge University Press. 1982. Pp. ix, 326. \$56.00.

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Communications

A communication will be considered only if it relates to an article or review published in this journal; publication is solely at the editors' discretion. Letters may not exceed seven hundred words for reviews and one thousand words for articles. They should be submitted in duplicate, typed double-spaced with wide margins, and headed "To the Editor."

TO THE EDITOR:

Upon receipt, early in June, of the February 1982 *AHR*, I was struck by some concluding remarks in Professor Griffith's article on President Eisenhower and wish to offer this comment.

The view that the period of Eisenhower's presidency was "among the most . . . peaceful and politically tranquil in this century" appears to be widely held, partly meaningless, and largely wrong.

To take the last point first, it is well to restate that the following events shook the nation in the decade indicated: the Iran crisis, Dien Bien Phu and the Geneva Conference, Guatemala, American pactomania, Mr. Dulles's barks of massive retaliation and agonizing reappraisal, Hungary, Suez, Sputnik, Lebanon, the Sherman Adams scandal, Cuba, Quemoy and Matsu, the beginning of the black revolution, threats to Berlin, McCarthy and his Ism, Cyprus, the U2 incident and resulting opening of the credibility gap and failure of the Paris summit conference, the Congo, the president's illnesses, the maltreatment of Mr. Nixon in South America, five million unemployed in 1958, the Rosenberg execution, the Aswan Dam story, and the cancellation of the president's visit to Japan.

That the view is partly meaningless follows from the logic that, if the period was among the most peaceful and tranquil, there must have been periods moderately peaceful, others moderately agitated, and yet others most agitated. It will be quite difficult to divide this century into four such categories and find several periods that fit into the first, permitting the Eisenhower era to be among them.

Nevertheless, it appears that many of our contem-

poraries look back on the 1950s as on a repetition of "normalcy." Even if we refrain from regarding the decade as the early Nixon era, in which later national disasters were permitted to germinate and ripen, we should not support what seems to be uninformed opinion.

Professor Griffith is, of course, aware of the disquieting events of the 1950s and mentions many of them. This makes his statement about tranquility more surprising. Most astounding, however, is his final remark that Eisenhower was a visionary. This is hardly the term to apply to a president who noted the apparent aimlessness of his nation and found it expedient to convene a commission to determine what American goals might be; it could, on the other hand, be a tribute to him for combining the best of two worlds: army life under the socialistic system that constitutes the army, capitalist royalties as a writer of bestsellers, and the gall to speak of "creeping socialism."

This is not to deny that, while confused and unsophisticated, Eisenhower was a fundamentally decent man and the best-loved president in American history; but readers of Professor Griffith's article will be hard put to find presidential words in it that were not platitudinous.

I conclude by suggesting that Professor Griffith, with tongue in cheek, has written an indirectly shattering indictment of Eisenhower's presidency.

KLAUS LOEWALD
*University of New England
Armidale, Australia*

TO THE EDITOR:

In the interest of dialogue within our beleaguered profession, I would like to comment on Charles C. Alexander's review of my book, *The Declassified Eisenhower: A Divided Legacy of Peace and Political Warfare* (*AHR*, 87 [1982]: 561).

Professor Alexander is entitled to use the traditional epithets of political disagreement or contempt, both misogynist (e.g., "strident," presumably

as in shrill woman), and partisan (e.g., "left-revisionist," presumably as in liberal-Democrat). But he is not entitled to dismiss the traditional materials of historical inquiry, the primary sources—the original documents—we were all once trained to honor, without some commentary.

Alexander asserts: "Blanche Wiesen Cook is the kind of historian who believes that truth remains embedded in mountains of classified materials until the unflagging efforts of determined researchers finally disclose what really happened." I am. They do. And in what Alexander calls an "awkwardly balanced book" I detail how. While Alexander says nothing about what is actually detailed, he complains that I spend over half the book on the prepresidential years. Why not? I wrote a book about Eisenhower's ideas on international relations, on war and peace and America's role in the world, as they developed over time. Eisenhower was president for eight years. His public career, during which his vision evolved, coincided with some stunning moments in U.S. economic and diplomatic history: The Nye investigations (Eisenhower's 1930s introduction to the issue of the emerging military-industrial complex); World War II; the renewed Cold War (when Eisenhower returned to Europe to build postwar unity on behalf of NATO). When political pundits level charges of chronological imbalance, they may be ignored. But when professional historians in our most esteemed journal do the same, we must all pause. What is the point? Are our students to believe only the presidency is worth our research?

Then what of the research? Alexander complains that I spend over seventy pages on one issue, Guatemala. I do. Because the documents relating to the CIA-State Department program (a four-year carefully orchestrated strategy to destabilize the Arbenz government) were for so long unavailable, it had been rendered a nonevent. Subsequently Stephen Schlesinger and Stephen Kinzer's *Bitter Fruit*, and Richard Immermann's long-awaited work on Guatemala also used these State Department documents released after years of appeals through the Freedom of Information Act. Additionally, there are several people now working on the vast archive, 98 boxes of Guatemalan government records sent by the CIA to the U.S. for propaganda purposes in 1954, then "lost" until I used them for the first time. These records, which now reside in the Library of Congress, are a mine of mostly unused primary research about the government that was overthrown. For thirty years there have been no final victories in Guatemala, and the killing goes on. Guatemala served as the model for other "political warfare" ventures, and only these declassified documents tell the story.

If these documents were not so valuable, so revealing and informative, politicians of all political

persuasions would be less concerned with keeping them closed and unavailable.

What is Alexander's message for our profession? Are we really to believe that our obligation to our craft is to limit our reading to each other's analyses, and to stop pestering the government for the primary sources of our discipline? Surely that is a radical view. Traditionalists trained to honor the documents, might however be at a loss to label it precisely on the "revisionist" spectrum.

BLANCHE WIESEN COOK
University of California,
Los Angeles

TO THE EDITOR:

In his review of Alfred Kelly, *The Descent of Darwin* (AHR, 87 [1982]: 199), Daniel Gasman primarily develops a polemic against those few pages in which Kelly seeks to refute Gasman's earlier *The Scientific Origins of National Socialism: Social Darwinism in Ernst Haeckel and the German Monist League* (New York, 1971). Gasman simply reasserts his own somewhat controversial thesis without taking the opportunity of entering into a scholarly dialogue. Problems of continuity and discontinuity in history are not so easily pushed aside: If the old Bölsche, as the principal popularizer of Darwin in Germany, supported the Third Reich at the end of his long life, so did the equally aged Karl Pearson in England, and no one would call him a precursor of National Socialism.

The considerable breadth of Kelly's approach to Monism should be addressed in any serious review of his book and in any consideration of the linkage between Monism and the Third Reich. Kelly's work is concerned with the popularization of Darwin in literature and in the schools, but above all with the influence of Bölsche's naturalism upon the workers' movement. Bölsche was close to socialism during most of his life, and Kelly, avoiding anachronism, is careful not to project a much later historical movement upon the Monism that flourished before World War I. His book concentrates upon the period before 1914 and on Bölsche's place in the history of the movement and its influence at that time rather than after the war, when the Monist movement per se mattered little. These fundamental emphases of Kelly's work should in fairness be made clear in a scholarly review.

STANLEY G. PAYNE
University of Wisconsin,
Madison

PROFESSOR GASMAN REPLIES:

It would appear that Professor Payne accepts uncritically Alfred Kelly's characterization of Wilhelm

Bölsche as a conventional socialist, and most likely would also take at face value Kelly's portrayal of Bölsche as a very moderate social Darwinist, wary of applying the Darwinian laws to society—in general a writer who popularized a kind of left-liberal or socialist naturalism very much at variance with an obscure minority of radically right-wing social Darwinists in Germany. Unfortunately, there is no historical basis for such assertions. The only socialism that Bölsche had affinities with was National Socialism and this would be as true for the 1890s as it was for the 1930s. The nature of Bölsche's socialism can be seen, for example, in a review article of Alfred Ploetz's, *Die Tüchtigkeit unserer Rasse*, which Bölsche wrote for the *Neue Rundschau* in 1896. Ploetz's book was intended primarily as an attack on the presuppositions of conventional socialism, arguing that socialist equality and Christian humanitarianism were eugenically dangerous, and despite its somewhat cautious approach, the book is of significance as anticipating National Socialist conceptions of society—Ploetz himself ultimately becoming a member of the Nazi party. Bölsche fully agreed with Ploetz that socialism in its conventional form was no longer acceptable and also recommended its reconstitution to reflect the now irrefutable Darwinian laws. To be sure, Kelly refers to this article (p. 115), but his account of it is far from accurate. Where Bölsche obviously supports Ploetz, Kelly conveys an impression of disagreement, and where Bölsche lavishes praise and admiration on Ploetz, Kelly has Bölsche pleading a case against social Darwinism. In this article, which runs more than ten pages, Kelly makes use primarily of only one paragraph, takes it out of context, and hence misrepresents Bölsche's general position. Such discrepancies between sources and what is reported are to be found all too frequently in this book so that the wide-ranging analysis that Professor Payne seems to be impressed by is in reality totally unreliable. For example, Kelly writes that Bölsche, in his biography of Haeckel, "takes his mentor to task" (p. 114) for social Darwinism, but I am unable to find any castigation of Haeckel in this book. On the contrary, Bölsche's biography is completely sycophantic. Kelly further maintains that Bölsche did not accept the common racial conceptions of the Darwinists (p. 117), but even a casual reading of Bölsche would confirm just the opposite. It was Bölsche's notorious idea of the bacillus-like character of the so-called lower races—hence the need for their extermination—that entered deeply into National Socialist terminology and practice. Bölsche's widely read book, *Vom Bazillus zum Affenmenschen*, as Werner Maser among others has shown (*Hitler's Letters and Notes*, pp. 245–46, and p. 381 n. 115), with its ominous and scarcely veiled allusions to the Jews as an antisocial and antievolutionary force, found a critical echo in *Mein Kampf*.

Kelly admits to none of this and denies any connection between Hitler and Bölsche. Nor can he grasp the radical nature of Bölsche's proposals to create an integrated racial community for the Germans so that they might gain ascendancy over Christianity and its presumed rebellion against nature.

Lastly, one should bear in mind that Bölsche was writing mostly "pseudo-scientific trash," as Hermann Glaser has aptly noted (*The Cultural Roots of National Socialism*, p. 187). Kelly, rather, maintains that Bölsche's writings were "spectacularly successful" and revealed an "elegant harmony" (p. 54), but I suppose that there is no accounting for taste.

As for Professor Payne's remarks about Karl Pearson, I am somewhat mystified. Bernard Semmel, in *Imperialism and Social Reform*, and more recently, George Mosse, in *Toward the Final Solution*, have both shown that Pearson developed a fully formed racial social Darwinism around the turn of the century. Why this should disqualify Pearson as a theoretical forerunner of National Socialism I am at a loss to understand.

DANIEL GASMAN

City University of New York

TO THE EDITOR:

Please permit me to respond to two problematical statements in Professor Robert Grathwol's review of my book, *Stresemann's Territorial Revisionism; Germany, Belgium, and the Eupen-Malmédy Question, 1919–1929* (*AHR*, 87 [1982]: 203–04). First, he quotes me as stating on page 82 that Stresemann's revisionist foreign policy was aimed at "unlimited *Weltpolitik*." This is a careless misquotation that is disturbing because it implies that I attribute infinite foreign policy ambitions to Stresemann. In fact, I state on page 82 that Stresemann aimed at "restoring Germany's *Weltmacht*." The statement is amply clarified in the second last paragraph of my book where I distinguish sharply between the aims of Stresemann and those of Hitler.

Second, Grathwol cites a "telling" remark from the preface of a recent book by A. W. DePorte to refute my conclusion that Stresemann's territorial revisionism undermined European cooperation. This is curious because DePorte, although focusing exclusively on German efforts to revise the eastern boundary settlement (pp. 33–35), basically corroborates my view that German territorial revisionism proved disruptive to Europe. It was misleading for Professor Grathwol to utilize a snippet from DePorte's preface in order to refute my interpretation without also noting that DePorte's text essentially supports it.

MANFRED J. ENSSLE

Colorado State University

PROFESSOR GRATHWOL REPLIES:

Professor Enssle's letter offers me some consolation. His objections to my review concern comments from my final section in which I try to place his study in a broader context than that of monographic research. I may thus hope that my efforts to offer a fair summary and objective critique of his detailed presentation in the preceding two-thirds of the review did not go entirely awry.

Enssle charges me with "careless misquotation" and of "misleading" the reader. I beg to differ. First, in the passage Enssle mentions, I do not quote him at all; I paraphrase him, with a reference to his text so that any reader may consult an example of the material I try to represent. Where he says "restoring Germany's *Weltmacht*," I use "unlimited *Weltpolitik*." A more judicious word might have been "overweening," but even without such a change, I find that my paraphrase does no violence to Enssle's overall interpretation, which puts Stresemann in the tradition of German diplomacy as practiced by the appointees of William II. It is true that in the ongoing debate on German foreign policy, this ambition to *Weltpolitik* and the drive to *Weltmacht* are often seen to continue from the Wilhelminian—or even the Bismarckian—Reich at least into the Third Reich. Whereas I make no such allusion in my review, Enssle himself makes the connection explicitly in his book when he speaks of Stresemann's revisionism as "one of the many 'bridges' between the Weimar Republic and the Nazi regime" (p. 202). What separates Stresemann, however, from the tradition in which Enssle places him (even while distinguishing between his aims and those of Hitler) are Stresemann's means, for Stresemann tried to win respect for his country through a diplomacy of adjustment and accommodation rather than to instill fear or to surprise his diplomatic partners with the "clever trick" (a label borrowed from Gerhard Weinberg, "National Style in Diplomacy: Germany," in *Oceans Apart* [1981], pp. 146–60), such as Enssle sees in the Eupen-Malmédy issue.

My reference to DePorte's comment on the diplomatic system in the 1920s was an effort to show that at this broader level of interpretation I have doubts about Enssle's basic assumptions, just as I have reservations about his detailed research. Had I been writing a review of DePorte's book, I might have taken the space to comment on his two-page discussion of Germany's policies in the 1920s. Enssle is correct: DePorte does judge this revisionism negatively. But DePorte's focus, his expertise, and therefore the area in which an appeal to his authority may have some validity, is on the system, not on Stresemann. I do not think it "misleading" or even "curious" to describe DePorte as contending that the interwar system established by Versailles was inherently unstable and unsound, independently of the actions of any one figure such as Stresemann. Given the system, DePorte argues, Germany's aims were bound to be disruptive. Therefore, I would add, the necessity to concentrate on differences in means. I quoted DePorte because I find that Enssle's unstated assumptions fit DePorte's characterization, that somehow "the touchstone of normalcy for much American thinking about Europe" was a restoration of the "old European system." In keeping with the spirit of this remark, Enssle attributes much of interwar instability to Stresemann's unwillingness to accept the given system. I disagree, as I think DePorte would. (See, for another instance of DePorte's attitude, his comment on the inevitability of change in that system [p. 32].) In contrast to Enssle, I see rather a fundamentally unstable system in need of revision, in which all the participants sought change to their own advantage and in which Stresemann sought to advance his aims through a policy of negotiated adjustment that took into account the legitimate interests of others while never surrendering what he saw as the vital national interests of his own country.

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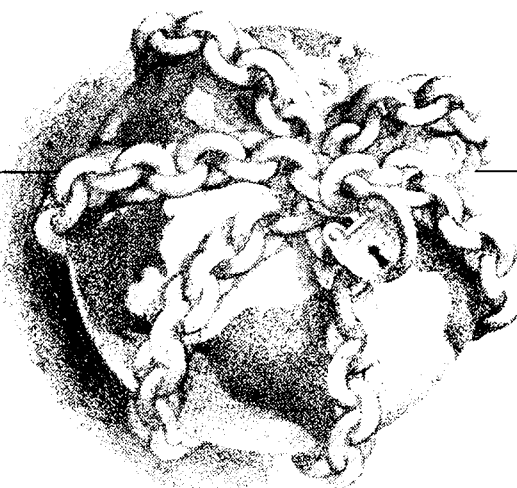
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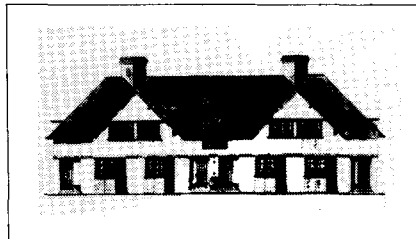
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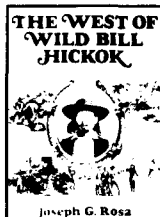
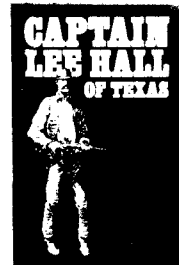
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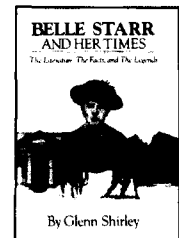
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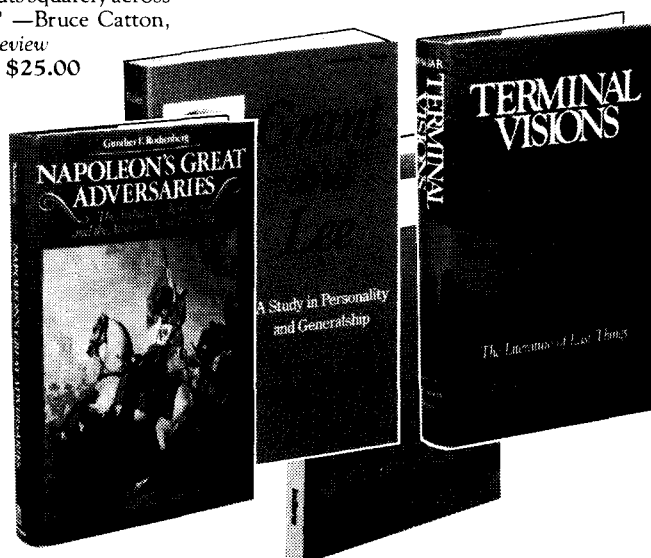
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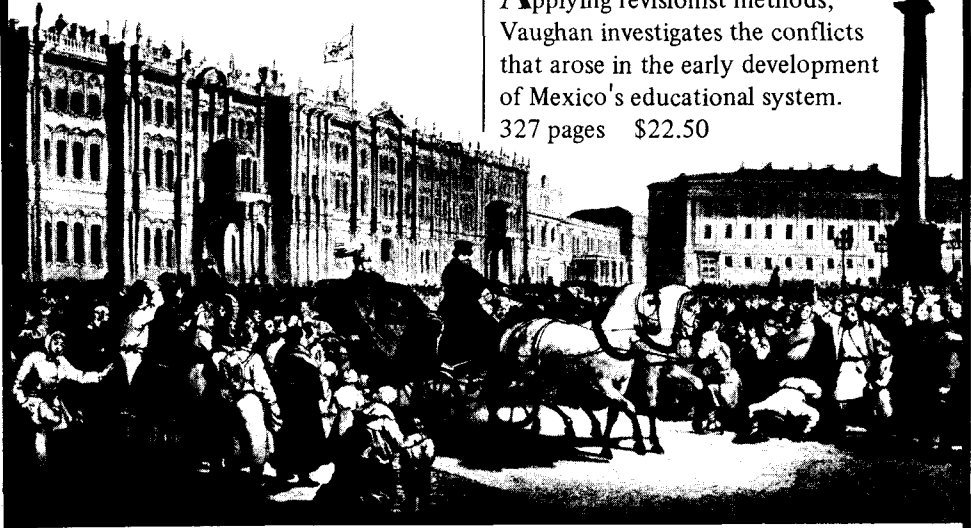
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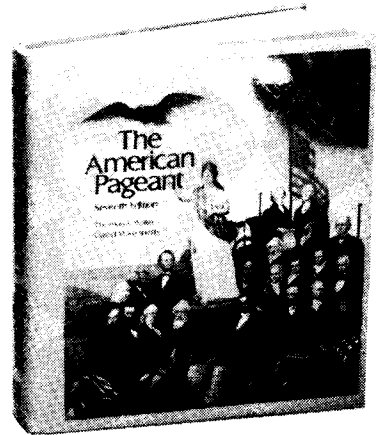
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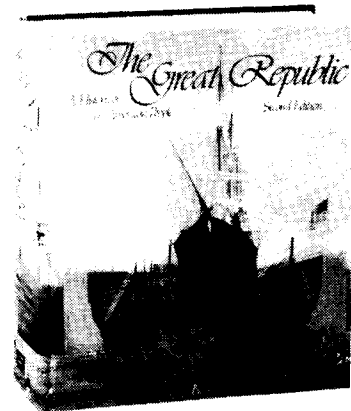
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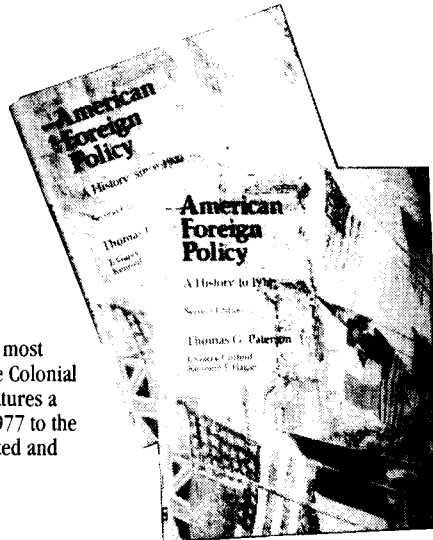
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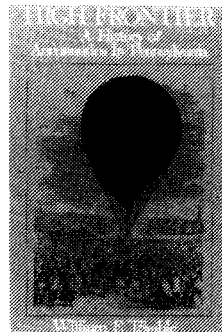
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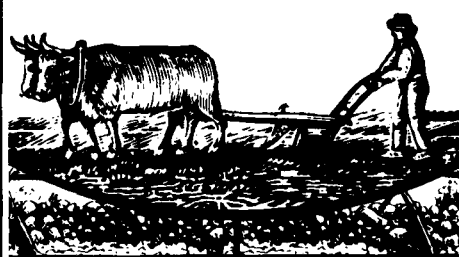


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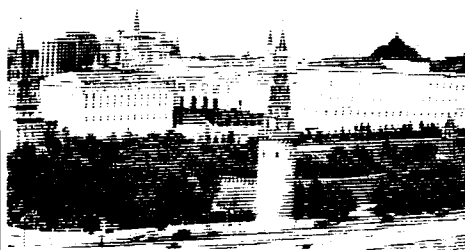
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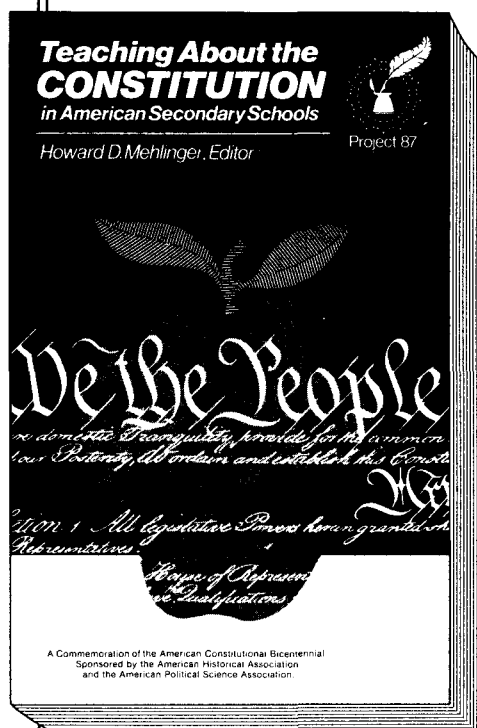
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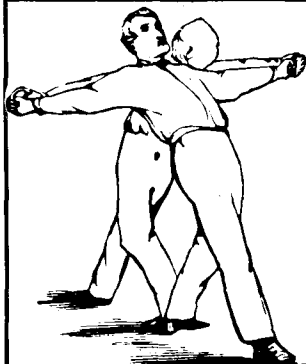
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